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COURSE OF

LECTURES ON THE BLOOD,

AND ON

THE CHANGES WHICH IT UNDERGOES DURING DISEASE,

Delivered at the College of France in 1837-8,

BY

M. MAGENDIE.

LECTURE IX.

Theories of bloodletting.—Compression of arteries and its effects.—Capillary vessels.—Discoveries of M. Poiseuille.—Circulation of blood in the capillary vessels.—Pressure of the fluid in veins.—Effects of variation in the quantity of the serum.—Theories of inflammation.— Increase of the cephalo-rachidian fluid.—Modes of examining the blood.—Proportions of serum and of clot.

GENTLEMEN:—We know, both by experiment and by theory, that the diminution of the extent of the circle traversed by the blood augments the internal pressure in the vessels. We have repeatedly satisfied ourselves that, by arresting the circulation in the right carotid, we cause a rise of the mercury in the instrument applied to the opposite artery; and that a fall ensues when we restore to the vessel its natural permeability. You saw compression of the aorta produce similar phenomena. When the static force of the liquid is annulled in any given point, the impulse of the heart really becomes, as it was fair to anticipate would be the case, more energetic in the portion of the arterial system still traversed by the blood; the moment we ceased to compress the vessel, the equilibrium became re-established throughout the entire arterial system. This law of the recovery of the equilibrium of pressure is of fundamental importance in the study of the circulation. Practitioners, who are unacquainted with it, fancy, in many cases, that by following certain rules in the employment of bloodletting, they shall obtain extraordinary advantages, but they sadly deceive themselves. When, for instance, they have a case of apoplexy to treat, they fix on the temporal artery, in preference to any other vessel, whence to deplete the vascular system. As that vessel is the nearest to the seat of the lesion, argue these reasoners, it must hold the cerebral circulation more immediately under its dependance.

No one would have had hardihood enough to deny the excellent effects ascribed to bleeding practised in those points of *election*, as they are called, while the theory which led to their adoption seemed so logically established. I confess that I myself formerly shared in the belief professed by the majority of medical men on the point; but, if you reflect on the matter, you will see that such a notion is utterly devoid of foundation. If the circulation were formed of a series of rings, mutually independent of each other, we might rationally open one vessel rather than another, according to the site of the disorder we had to combat; but the chain formed by the arterial tubes is perfectly continuous throughout the frame. The pressure cannot diminish at one point without doing so, to the same extent, in every other. Whether you bleed from the temporal or the tibial artery, the effects will be mechanically the same in respect of the circulation of the brain. The preference given the former of these vessels is justified by its superficial position, which renders it easily accessible; but, as regards the therapeutical influence of its division, it can lay claim to no real superiority. If you represent by 5 the diminution of pressure in the temporal, you must represent by 5 also that in the tibial artery. The hæmodynamometer applied to the two vessels would show the same fall in the mercury.

Some days ago I read the article "Saignée," in one of our medical dictionaries, enjoying the highest repute, but was, I confess, totally unable to comprehend the strange notions propounded in it on the subject of the movement of our liquids. What is to be understood physiologically, I would beseech you to tell me, by a *revulsive* or a *derivative* bleeding, on which the writer descants so learnedly? For my part, I knew it not before I met with the article in question, and am at a still greater loss, now that I have become acquainted with the various hypotheses whereby an attempt is

therein made to prove that the opening of one vein is preferable to that of another. If there be a sensation of weight or fulness in the head, we are told to open the saphena vein, because the blood will be directed thus from the head to the feet, and the excessive plenitude of the cerebral vessels removed.

Such are the precepts written and professed by our most distinguished practitioners, just as if vacuity of one portion, and repletion of another, could coexist in a system of tubes freely communicating with each other. Again, the question is seriously discussed, whether it is advisable to bleed from the foot in inflammation of the digestive tube. It is decided, at length, in the negative, on the ground that the blood, rushing in quantity towards the opened vein, would be obliged to pass by the intestines on its route, and would so increase the activity of the inflammatory element. Further, it is a common inquiry, whether, in cases of pneumonia of the right lung, we should bleed at the right side; and in cases where the left organ is affected, at the left? Opinions are still divided on this point; a good number of practitioners, however, will answer you in the affirmative, and regulate their practice accordingly. But there is another method of bloodletting, which is reserved for great and important occasions, and to which very honourable and conscientious men accord incontestable efficacy; I mean *cross* bleeding. Suppose a case in which a variety of therapeutical measures have proved unavailing, while the disease continues to gain ground; a case, in short, in which ordinary art is powerless. What is to be done under such circumstances? A consultation of medical celebrities is, of course, held, and upon what do you suppose the deliberation sometimes turns? Upon the propriety of opening a vein in the right arm, at the same time as another in the left foot!

I was actually, some while past, one of a consulting party, among whom this proposition led to a discussion worthy of taking rank with the richest scenes of comedy. I do not seek, Gentlemen, to excite your hilarity; the patient was a dying man, who had but a few moments to live. Who would have dreamed that in an age which judges with such severity the prejudices of our fathers, men could be found not only to tolerate, but actually to extol such superannuated practices? Is there, I would ask, such a very great difference between the employment of amulets, which have supplied such a capital butt for our gibes, and the confidence attributed to bleedings, the jets of which cross each other in the form of an X? The day will come, and may it not be far removed, when the profession will refuse to believe, that in the year of grace, 1837, conscientious practitioners of the capital of France, were found to countenance such monstrous absurdities. But, on the other hand, consider the excellent practical deductions to which a correct knowledge of the mechanism of the circulation has led some observant men. Among others, flowing from the experiments you have seen, is the method proposed by a distinguished obstetrician for the treatment of uterine hæmorrhage following parturition. It occasionally happens, as you are well aware, after the separation of the placenta, that so abundant and obstinate discharge of blood takes place, that cold astringent injections, the application of topical refrigerents of all kinds, and, in a word, every plan recommended in such cases, utterly fail in stopping it. And, in truth, any one who has seen the size of the venous orifices, that pour the liquid into the interior of the uterus, can have no difficulty in comprehending the powerlessness of the processes so improperly termed hæmostatic. The point, then, was to prevent, in some way, the uterine vessels from receiving the blood propelled by the left ventricle; and this M. Baudelocque has effected by pressure of the aorta through the abdominal parietes. The laxity of those parietes is so great after the expulsion of the fœtus that they may easily be depressed either with the fingers, or by the help of an instrument, which is by far the more advisable plan.

The suspension of the flow of blood through so voluminous a vessel as the aorta has a double effect: it immediately arrests the hæmorrhage on the one hand; on the other, it diminishes the extent of the circulation, and, consequently, increases the pressure in the arteries situated above the compressed point. As the brain receives more blood it is evident that the cerebral functions will acquire fresh activity; and, accordingly, we find that the patients recover their consciousness, that the face reassumes its natural colour, and that the prostration gradually ceases.

M. Baudelocque has rendered a real service to humanity by the suggestion of this plan; and the recompence voted him by the Institute shows that we know how to estimate its importance. Again, it has been noticed, that one of the chief means of restoring an individual in a state of syncope to consciousness, is to place him in a horizontal position; in that attitude the circulation requires less effort than in the vertical position, which easily explains its favourable effects. Perhaps we might act more directly on the encephalon by compressing a large trunk, such as the brachial, the femoral, or even the aorta, in these subjects. According to the principles of hydrodynamics, the pressure of the cerebral vessels would become more powerful, and the nervous system consequently receive an increase of excitement. I have never tried

this plan, but, from the result of our experiments, I can scarcely entertain any doubt of its efficacy.

We have so far studied the static force of the blood without endeavouring to form any separate estimate of the influence of the respiratory movements on arterial pressure. We might for this purpose, in imitation of M. Poiseuille, open both sides of the chest extensively, by removing the sternum, and, by means of artificial respiration, keep the animal alive long enough to note the oscillations of the mercury in the hæmodynamometer. But I prefer slightly modifying his method of conducting the experiment; instead of putting a sudden stop to the play of the thorax, I will modify it gradually; we shall in this manner be enabled to follow all the phases of the phenomenon. The instrument is applied to the carotid, and the scale marks 75—105, 70—105. A tube has been fitted to the trachea, so that if it become necessary, we may practise artificial respiration.

M. Poiseuille suggests that it would be well to open and shut the cock alternately, which regulates the passage of the air into the trachea; I will act on his suggestion, as it will allow us to estimate the effects of forcible efforts of expiration. The mercury still stands at 75—105 mill. I close the cock, and we have 25—55, 15—50, 10—50, 10—55 mill. I open it, and the mercury marks 100—150, 95—160, 90—155 mill. Hence it appears that the column, which had fallen considerably from the effect of the obstacle to pulmonary expansion, rose the moment the animal had performed some full expirations. Now, that the chest expands and retracts freely, the mercury regains its former level. What we have next to do is to prevent one side of the chest from acting; for this purpose I open the left pleural cavity with the *pleura-borer*. The corresponding lung collapses, and in order to keep the orifice open, I introduce a caoutchouc tube into it. The animal makes violent and fruitless efforts to breathe. The column rises to 90—125, 80—145, 90—160, 85—165 mill. The sudden ascent of the mercury is perfectly conformable to theory, and cannot, therefore, in anywise surprise you. I next proceed to open the other side of the chest, so as to put a total stop to the movements of respiration; and, with this view, now make a puncture through the walls of the cavity with the *pleura-borer*, an instrument I devised for the purpose.

The sibilus you have just heard shows that the air enters the cavity of the chest. Both lungs are now in a perfectly collapsed state. The scale gives 40—85, 50—90, 45—85 mill. The cessation of respiration consequently induces a notable diminution of arterial pressure. You must have remarked that an involuntary discharge of urine and fæces takes place in experiments, whenever the cerebral action of the animal is modified. This has happened in the present instance. A similar occurrence follows concussion or lesions of the encephalon in the human subject. The contraction of the bladder and rectum augment in proportion as the nervous influence diminishes. I will now perform insufflation through the trachea to replace the action of the thorax. The dog is not dead yet, though both his lungs have now been in a complete state of collapse, from the elastic contraction of their tissue. My assistant keeps the bellows in movement. The column continues to fall; it oscillates between 20—70, 20—60, 45—80, 30—50, 15—25 mill. The sounds of the heart are scarcely to be heard on applying the ear to the precordial region. The animal must inevitably perish, and he is too weak to admit of our pursuing the experiment further.

I have shown to demonstration, Gentlemen, that the elastic tubes known as the arteries, perform a merely passive part in the great function of the circulation; but their size is far from being as easily ascertained by our experimental investigations, as the nature of the part they play in that function. The trunks and branches of a certain size may be easily studied with the naked eye, but such is not the case with their ultimate ramusculi, which are of extraordinary tenuity. According to the latest microscopical researches, it would appear that the diameter of the capillary vessels may not exceed the 1-800th of a millimetre. There is possibly some exaggeration in this estimate; but all are agreed that the diameter of those exquisitely minute ducts does not exceed the 1-150th or the 1-200th of a millimetre. Now the blood is obliged to traverse myriads of canals of this diameter in order to pass from the arteries to the veins. This is a state of things which is not met with in any of our machines; it has, consequently, not been studied by natural philosophers. Physiologists, alone, have directed their attention to it. Unfortunately they have taken so erroneous a view of them, that if we desire to come at any accurate knowledge of these hydrodynamic phenomena we must begin by supposing (which is actually the case) the science to be in a complete state of barbarism as respects it. M. Poiseuille, and several other experimentalists, engaged in the inquiry with this impression, and accordingly their labours form a new era in the history of the circulation.

The first problem that presents itself for solution is this: is the heart's impulse propagated from the arteries to the capillaries? As those two classes of tubes are throughout continuous, it is extremely probable that such is the case; for, speaking in a mechanical point of view, it seems likely

that the same force which acts at the origin of a system of ducts will continue its action to their termination. Reasoning is in favour of this opinion. I shall not recur to the proofs which may be adduced in its support, for I have frequently had occasion to expose them to you; and, besides, I am anxious at once to attack the experimental part of the question. Let it suffice for us to know that the thing is not only possible, but that it has in its favour all the chances of probability. It does not require to be a profound anatomist to ascertain that a free communication subsists between the arteries and veins, through the medium of the capillary vessels. Push an injection into the spleen, or kidney, by the arteries, and it will return by the veins. None will deny this fact. But the litigated point respects the force that causes the fluid to move through the innumerable intervening canals. Well, you will see that the heart acts in the living subject as the piston of this syringe on the dead body, and that they who refuse to acknowledge the reality of its action in this way are, in truth, refusing to admit the testimony of experimental observation. It is a curious point for inquiry, to determine in what manner the impulsion propagated from the arteries to the veins is modified by being spread over a larger space. Does it retain its original intensity? No. The same character? No. Then a new series of researches is called for. Although the fundamental phenomenon be literally the same, it is necessary to take into accurate account the peculiarities proper to each locality. The further we advance from the central organ of impulsion, the more we find the rapidity of the flow of the blood diminished. The jerking movement of the arterial blood is very evident in the large trunks, it becomes less so in the secondary tubes, and is totally changed into a uniform motion forwards in the capillary vessels. Once arrived in the veins, the column of liquid moves extremely slowly. The walls of these vessels are scarcely pressed on by the current that traverses them, and are collapsed in the ordinary state; it is for the purpose of causing an artificial distension of them, by the accumulation of a greater quantity of blood than usual in their cavity, that we apply a bandage round the arm, above the point where we intend puncturing the vessel. The slackened flow of the blood in the capillaries is a point which had not been fully explained previously to the latest experiments of M. Poiseuille. To that young physiologist we are indebted for some most interesting microscopical observations on the manner in which the liquids conduct themselves while traversing the capillary vessels.

Among the most important facts he has discovered, are the following. Whenever a liquid moves in a tube, a certain layer of it adheres to the walls of that tube and remains motionless. If the course of the blood be examined under the microscope in an artery which has coats sufficiently thin to admit of the passage of luminous rays, the rapidity of the movement of the globules is found to be greatest in the axis of the vessel. This rapidity diminishes gradually as we pass from the centre to the circumference of the vessel. There is a transparent space next to the internal tunic, which varies in breadth from the tenth to the eighth of the diameter of the tube, and is filled with the serum of the blood. But, as it may probably have occurred to some of you, if this fluid be transparent, how can the reality of its presence be ascertained? How can it be shown that there is no optical delusion connected with it? This is done through the movement of the globules. Some of these are occasionally detached from the central current, and, in consequence, brought nearer the peripheric motionless stratum of fluid, and, at the same instant, this movement becomes much less rapid. Such among them as are jostled by their neighbours, are dashed against the walls of the vessel, and become stationary. Hence a translucid liquid, holding them in suspension, really exists, and communicates its immobility to them. In a large vessel these different degrees of rapidity of the fluid molecules have hardly any influence on the movement of the general current. But if you suppose a tube of considerably less diameter, a greater relative quantity of the liquid will be motionless, and consequently the central column moves in a comparatively narrow area. When the calibre of the vessel is still smaller, the stratum adhering to its walls obstructs its interior almost completely, and a mere filament of fluid can with difficulty force itself a passage in the centre. Finally, when the degree of tenuity becomes extreme, the tube ceases almost wholly to be permeable to fluids.

I have already stated to you my conviction that if it were possible to find inorganic tubes of such exquisite delicacy to experiment on, we should not succeed in forcing distilled water into their interior; and yet the blood, a fluid remarkable for viscidity, and which holds myriads of insoluble globules in suspension, circulates freely, under the influence of a slight impulsion, through canals of this prodigious tenuity. Unfortunately, such is the perfection of the processes employed by Nature in this marvellous system of animal hydraulics, that we are better able to admire than to comprehend it. The composition of the liquid, as I have already said, plays a most important part in the facility with which its molecules traverse either inert or living tubes. Water containing acetate of ammonia, tartrate of antimony, and potass, albumen, &c., passes with greater ease than distilled water. Now,

chemical analysis proves that the blood holds several salts in solution,—an important circumstance, which shows at once that that fluid may be expected, on physical principles, to conduct itself differently from a simply aqueous liquid. But, on the other hand, the viscidity of fluids is one of the most powerful obstacles to their passage through tubes of very narrow diameter. Olive oil injected into the veins causes rapid death by the obstruction of the capillary rete of the lungs. The same effect is produced by mucilaginous liquids, such as gum-water, syrup of dextrine, cerebral emulsion, &c. How, then, is it possible for the blood to traverse such infinitely minute canals? Must not its viscidity contribute to retard or even to arrest the circulation completely? Far from this, the very property which is, in the case of inert tubes, most unfavourable to their permeability, is an indispensable condition of the performance of the hydraulic phenomena of living tubes. The blood is even more viscous than those substances, which, when injected, cause immediate death. Deprive it of its viscidity, and you convert it into a fluid unfit to circulate in its natural canals. Must we, then, admit that there is an opposition between physical and vital laws in this instance? I shall leave you to decide for yourselves. If we could make artificial tubes possessed of the same properties as the arteries, with porous walls, and lined with a smooth, unctuous membrane, perfectly fitted for contact with the blood, we might then be entitled to draw such a conclusion, if the results were the same as they now are with our feeble imitations of the arteries, made of metal, glass, or caoutchouc. Between such instruments and the living vessels no just comparison can really be instituted. This is the knotty part of the question.

Observe, I beg of you, that I adduce in support of my assertions the authority of such facts only as you have yourselves been eye-witnesses of. You have seen me deprive the blood of its coagulability either by removing its fibrine, or by alkaline injections. You saw that morbid transudation followed; that the materials of the liquid were effused by exhibition. Hence the composition of the blood cannot be modified, without derangement of the circulation ensuing. Again, you saw me adapt a caoutchouc tube to an artery, and cause some blood to enter its cavity; the liquid became solid in its passage through it. Hence the nature of the walls cannot be changed without disorder occurring in the circulation. The nature of the intimate relation subsisting between the living tubes and the liquid that traverses them is a profound secret. We may modify the physical properties thus intimately bound up with the normal state of the circulation, but we shall strive in vain to endow dead matter with similar attributes: a mysterious and unknown something will always be found wanting in our imitations. It would, therefore, be a very serious error to apply all that is known concerning the passage of liquids in inorganic capillary tubes, to that of the blood in the living capillaries. There are common phenomena in each case, it is true; but there are others totally different. All must be taken into consideration if we expect to learn anything positive and non-conjectural on the subject.

The adherence of a motionless stratum to the walls of the capillary vessels, is a capital fact, inasmuch as it explains how the course of the blood becomes slackened in those tubes. In order to surmount this obstacle the left ventricle is obliged to dispense a part of its contractile force; but its power is far from being exhausted, for it extends its influence even to the veins, as I shall demonstrate to you. Circumstances exist under which the ventricular impulsion is quite as evident in the veins as in the arteries. Now, this impulsion cannot pass from the latter to the former without affecting the capillary system in its way. If the movement of the blood in the veins were due to the action of the capillaries alone, the motion of the liquid in them would be uniform, and not in harmony with the causes which increase the force of movement of the arterial blood. The degree of the heart's energy, the respiratory movements, the volume of the liquid, would all of them be without influence on the venous circulation. Now, we have the testimony of daily observation in evidence of the contrary. It is positively ascertained that whatever acts on the flow in the arteries, acts also on that in the veins. My experiments had already demonstrated the existence of a close relation between those two great systems of tubes, when M. Poiseuille, by his late researches, gave us a mathematical solution of these important questions, and proved the futility of the theories founded on an assumed action of the capillaries. Before investigating the causes which produce a variation of pressure in the veins, it will be necessary to ascertain the exact degree to which that pressure exists in the normal state. This we will endeavour to do with the hæmodynamometer. The left jugular vein has been laid bare for the purpose in this dog. The extremity of the tube is introduced into an opening made in the vessel: two ligatures are applied; one fixes the upper end of the vein firmly to the instrument; the other ties the inferior end so as to prevent reflux of liquid. Now that all is arranged, I proceed to turn the cock which separates the blood from the subcarbonate of soda. The rise of the mercury will indicate the amount of pressure to which the coats of the veins are subjected. The mercury now begins to oscillate, the scale marks 15, 20, 14, 18, 15,

17, 18 millimetres; so that, instead of remaining stationary, the column falls and rises alternately a few degrees. By watching the movements of the thorax, and following the pulsations of an artery with the finger, we soon ascertain that the greatest elevation of the mercury corresponds either to an expiration or a contraction of the left ventricle. You remember that blood moves in the arteries with greater force during expiration than inspiration: this single experiment would, therefore, suffice to prove the influence of the pressure of the arteries on that of the veins. If the blood, when once it reached the capillary system, were kept in activity by a motor power belonging to that system alone, it is evident that its movement would be invariably uniform. The animal begins to struggle violently, and accordingly we find that a sudden rise of the mercury to 35—40 mill. takes place, showing that some accidental agency has interfered with the ordinary progressive force of the blood. The respiration again grows calm, and it falls to 15—18 mill. Hence the blood in the veins is manifestly influenced by the mechanical agents which augment its force of impulsion in the arteries. Observe, too, that inasmuch as the ascent of the liquid is effected uninterruptedly, there must be some other cause of movement in addition to expiration, and the action of the left ventricle: that cause is to be found in the elastic reaction of the arterial tunics, consequent on their dilatation by each wave of blood propelled forwards by the heart. As for the proper action of the capillary vessels, be assured that such action exists not in nature, but solely in the imaginations of those physiologists who have described it.

I will next apply the instrument to the femoral vein near the crural arch: the extremity of the tube is directed as before towards the capillaries. The pressure is uniform, as you are well aware, in the arterial system generally, but it varies in each quarter, and, indeed, in each vessel of the venous system. I can consequently make no *a priori* affirmation respecting the amount of pressure which the femoral vein supports. The actual application of the hæmodynamometer alone can furnish an accurate estimate of it. The cock is now turned, and the mercury marks 50, 60, 50, 45, 50, 55, 58 mill. The pressure is, therefore, clearly much more considerable in the femoral than in the jugular vein. There is another important point to be noticed here; although the animal struggles violently from time to time, yet the level of the column remains nearly stationary. You observe none of those sudden and rapid ascents to which I drew your attention in the former experiment. This proceeds from our acting on a vessel placed at a considerable distance from the chest. You already know that the influence of deep expiration is much more marked in the neighbourhood of, than at a distance from the pectoral cavity. I shall have occasion to return to the consideration of this phenomenon, which I simply notice for the present. I long ago established the fact of the increased force of progression of the venous blood during deep expiration. I opened a vein with a lancet, and at the same time induced great respiratory efforts on the part of the animal; I then distinctly saw that the jet of blood diminished or increased according as the air entered or was expelled from the chest. But, although the results of M. Poiseuille's experiments agree with those of my own, yet I have much pleasure in acknowledging the immense utility of the hæmodynamometer in the examination of these physical questions. Any instrument that gives the study of a phenomenon a character of precision is beyond a doubt an invaluable acquisition. We will continue these experiments at our next meeting.

The point in the history of the blood about which we are at present more immediately interested, is the effects induced in the economy by variations in the relative quantity of the serum. If we could succeed in determining, even in an approximative manner, the influence of those changes on disease, and on temperament, we might assuredly lay claim to the honour of having done some service to pathology. But, Gentlemen, this question is not one of those that may be solved in a single lecture, nor in two, nor in three; it requires to be examined in various points of view, and calls for the evidence of numerous facts of an accuracy that none can gainsay. When we shall have studied this branch of our subject, we will turn to the chemical composition of the serum and clot, which, it cannot be denied, still requires minute investigation, although it has already been laboriously studied. We have hitherto done no more than receive specimens of blood into vases, examine them, and the changes they undergo therein, compare, as well as we could, the proportions of their liquid and solid constituents, and inquire if the phenomena of life can continue to be accomplished with such and such quantities of serum and clot. You have already seen, by the serious modifications induced in the state of the economy by certain diversities in their proportions, that we are not without plausible motives for investing this question with the importance we have done. Let me recall to your minds the case of uterine hæmorrhage of which I spoke to you in my last lecture, following abortion produced by criminal practices. You cannot have forgotten the consecutive symptoms, such as the intense peritonitis which supervened at the end of two days, accompanied with most violent pains and embarrassment of the respiratory system, and which terminated by death in less than

four and twenty hours. This case is worthy of close consideration; for, setting aside the signs proper to that fatal affection, we may ask what is peritonitis? what are its mode of origin and first cause? In general the answer is, that it is an *inflammation* of the peritoneum, that the mode of vitality of that membrane is changed by *irritation*; that this *irritation* accumulates the blood in the capillary vessels, which were previously impermeable by it, and that both an increase and a modification of the materials of exhalation are consequently produced. When these different suppositions, and a variety of others, have been more or less carefully enumerated, it is presumed that all has been said; nevertheless, the real question has not been so much as glanced at. For my own part, though I admire the ingenuity of those pathologists, who have made peritonitis one of the inflammations *per eminentiam*, I cannot quite accede to the correctness of their opinion. We found in the blood of the woman to whose case I now especially refer, a most remarkable proportion between the serum and clot; there were 85 parts of serosity to 15 of coagulum. This single fact was in itself enough to suggest to our minds a very different explanation of the disorders under which she succumbed, from that afforded by the doctrine of inflammation; and we had no difficulty in establishing the existence of certain relations between this occurrence and the phenomena daily observed in our experiments. Among other things, you saw that there was a most striking resemblance between the lesions of the lungs in our patient, and in those occurring in the pulmonary organs of the animal submitted to a series of successive bleedings. But I must avail myself of the present opportunity to point you out a physical phenomenon long mistaken by medical men for a pathological change. In consequence of the greater or less liquidity and diminished consistence of the blood in certain cases, it becomes infiltrated from the capillary vessels into the pulmonary cells, accumulates therein, and constitutes the effusions known under the name of *hypostatic pneumonia*, which is, in truth, a simple effect of gravitation. Accordingly this condition is always met with in the most dependant parts; and inasmuch as the horizontal decubitus is that usually affected by patients, the postero-inferior parts of the lung are its ordinary seat, while the antero-superior portions are still healthy and crepitating. If we could open our dead bodies at an earlier period after death, we should not meet with infiltrations of this kind so frequently; a fact we have fully ascertained in the autopsies of animals destroyed in our experiments. Further, the patient in his last moments compresses the lung by his efforts to breathe, and the blood imbibed into the parenchyma of the organ traverses its external serous covering, and goes to form the collections of serum and of colouring matter, such as we found in the pleural cavities of the patient under consideration. We discovered in the abdomen of the same subject a yellowish liquid containing albuminous shreds, yellow colouring matter, and something having a certain analogy to the soft flabby clot formed in the blood removed by venesection. I examined this liquid under the microscope, and discovered a considerable quantity, not of globules, but of particles of a singular form held in suspension, and intermingled with filaments of various lengths. I presume that it was nothing more than blood modified as regards the mode of coagulation of its fibrine, which had transuded through the serous membrane. I detected no globules of pus; and you are aware that nothing is more easy than to distinguish purulent globules under the microscope, when one is at all in the habit of observing them,—so that most decidedly none existed. Here is a piece of intestine which I have had conveyed hither, in order to show you the false membranes. In this organ, as in the lung, we have a very curious phenomenon exemplified; I mean the separation and deposition of a liquid under precisely the same conditions. This, too, is a cadaveric phenomenon, analogous to that of which I have already spoken to you. We may now, I conceive, regard it as an established fact that the curious series of phenomena observed in this woman were developed by the superabundance of serum in her blood. You must acknowledge that I by no means twist the facts so as to make them subservient to my opinions, but that, on the contrary, the latter have actually been modified by the former.

I now proceed to lay before you a fact which will give you a just idea of the difficulties encountered in these investigations. The blood you see in this vase was furnished by a young girl who had a slight uterine hæmorrhage after a miscarriage, which she denies having produced artificially; the blood has almost wholly formed into a solid mass, and presents a few drops only of serum. The same phenomenon, to which I have already adverted, occurs in this instance: the clot has imprisoned all, or great part, of the liquid in its cellular structure; it will subsequently undergo a sort of contraction, and then allow of the escape of a greater or less quantity of serum. The manner in which the coagulum forms, evidently exercises considerable influence on the visible proportion of serosity; hence, before forming any opinion on its real quantity, we must ascertain how solidification has proceeded. These reflections on the serum remind me that when I commenced my medical career, imbued with the prejudices of the schools, and just such a novice as men usually are when they give up attending lectures; when, too, like my

brethren, I paid my tribute to scholastic dogmatism; that is, I believed, in inflammation, irritation, and the rest of it, as in so many articles of faith; they remind me, I say, that, even at that early period, these questions excited my attention. You shall hear how I was led to their consideration. It was at the time I allude to an acknowledged point of doctrine, that the abundance of serosity acted on the blood by *modifying its tendency to inflammation*, in somewhat the same manner as water added to alcohol prevents it from *inflaming*. Here, Gentlemen, the word is used in its true signification. I had, as it happened, set about repeating the experiments of Sir Benjamin Brodie, now one of the first surgeons in England, on the ligature of the ductus choledochus. The animals on whom I practised the operation died, without exception, of peritonitis. With a view of preventing this disagreeable result I practised a copious bleeding before the experiment, fancying, in conformity with the notions then prevalent, that I should thereby infallibly put a stop to the development of inflammation,—the inflammation nevertheless appeared with even still greater intensity than before. Subsequently I injected water in the room of the blood withdrawn, but in every instance peritonitis supervened with greater violence than before, and proved rapidly fatal. At the present time, when more correct notions on pathology have replaced those of former days, it appears to me that the more the blood abounds in serosity the more probable it becomes that the consecutive exhalation of the serous membranes will be abundant; and, hence, that, to use the orthodox language, inflammation will be more violently developed. Now, this fact, alone, shows what fatal consequences may be the result of a fallacious theory, founded on an imperfect conception of the morbid phenomena occurring in the body. I do not hesitate to assert that the anti-inflammatory bleeding ordinarily practised before capital operations may frequently, according to the constitution of the individual undergoing them, help to determine the serious accidents observed to follow those operations. I recommend you strongly to take a note of this proposition, and to watch with attention the issue of cases in which such prophylaxis has been adopted. You will, no doubt, find exceptional cases, but in the majority I make no question you will have reason to admit the justness of this view. The whole forms a subject for inquiry, which, though of great interest, has not yet been examined by any one. I myself long upheld contrary opinions to those I now maintain, but I willingly sacrifice my vanity and acknowledge my error; if all were as ready to do so the progress of our science would be much more rapid. This recalls to my mind certain experiments of this kind that I made in the hospital on the human subject. Formerly, when an individual was seized with the terrific affection known under the name of hydrophobia, the mode of treatment adopted was as follows; and, indeed, these charitable plans were employed at no very distant period from our own:—The wretched sufferer was either stifled between two mattrasses, or bled from the four extremities, and allowed to perish of hæmorrhage, or he was put into a sack and thrown, sack and all, into the river. These methods of treatment, originally adopted from ignorance, were continued up to our own time, from the most culpable indifference. You are aware, without doubt, how this horrible disorder is communicated, and are acquainted with its principal symptoms. The most characteristic of these is a spasmodic contraction of the constrictor muscles of the pharynx; the patient becomes furious at the sight of water, and even of any polished surface; a thick and abundant foam flows incessantly from his mouth, and if he were not held down he would rush on the bystanders to attack and bite them; he loses the power of recognising either friends or relations. I felt anxious to try whether I might not, by injecting water into the veins of one of these patients, succeed in soothing the nervous excitement under which he suffered; I was partly successful, for, after I had injected nearly two litres of water into the circulation, the patient became calm; he even asked for water, and actually drank,—a most remarkable circumstance; for, as its name indicates, the disease is characterised by an aversion, a horror of water. I was not fortunate enough to save his life ultimately, nevertheless I had the satisfaction—the most intense of my life while it lasted—of prolonging it for six or seven days, during which he lay in a state of perfect tranquility; whereas either men or animals affected with rabies ordinarily perish in thirty-six or forty hours, in the most terrible anguish. My patient died, as I said, on the sixth or seventh day, and presented the curious phenomenon of articular dropsy in several joints, no doubt in consequence of the enormous quantity of water injected into the veins. This last fact is exceedingly important in respect of the present subject of our investigations; there is a very strong analogy between these hydrarthroses and the effusion that took place into the peritoneum and pleura of the patient of whom we have been speaking. I will make some experiments on this matter, and inform you of the results. While on the subject of serous effusion I may illustrate it still further by the case of a woman admitted some time past under my care at the Hôtel Dieu. The most remarkable symptoms she presented were these:—Complete loss of intelligence: partial abolition of sensibility;

the patient's countenance expressing slight suffering when the skin was pinched; movements obscure and rare; decubitus dorsal; no deviation of the face; stertorous breathing; pulse natural. These various symptoms had not made their appearance successively and at intervals; on the contrary, the patient had suddenly passed from a state of health to the most dangerous one I have just described. The diagnosis in this case was far from being easy; the suddenness of the attack, and the nature of the symptoms, immediately drew my attention to the encephalon; the absence of hemiplegia, and of other symptoms limited to one side of the body, made me reject the supposition of hæmorrhage into one of the hemispheres into the brain; it might be that an effusion of blood had taken place into the ventricles, or into the pons varolii, or into some other part situate on the median line; the combination and general characters of the symptoms induced me to suspect the existence of another lesion. You are aware that a layer of liquid, situate between the pia mater and arachnoid membranes, surrounds the nervous centres. The ancients must have had some knowledge of this fluid, at least such a supposition is warranted by various terms employed by them in describing the anatomy of the brain, such as the *aquaduct* of Sylvius, the *pons* Varolii, the *valve* of Vieussens, &c. Cotugno, an Italian anatomist, noted its existence in the dead body, but modern observers had completely overlooked the fact, and considered the liquid either as a cadaveric phenomenon, or as a product of morbid exhalation. Such was long my own opinion also. It was while engaged in making experiments on the section of the roots of the spinal nerves, that I established, for the first time, the constant presence of a liquid round the nervous substance. I also proved, in contradiction to the ideas of Bichat, that the cerebral serosity was not contained in the cavity of the arachnoid, but in the tissue subjacent to that membrane. As the notions of the old observers were of the vaguest kind respecting this liquid, and as physiologists of our own time paid not the slightest attention to it before my researches were made public, I think I am entitled to the credit of the discovery. But this is not the place to argue questions of priority; I have simply to state the uncontested fact, that a stratum of liquid exists under the arachnoid in the normal state, to which, from its situation, the name of *cephalo-rachidian* has been applied. If this liquid be suddenly exhaled in excessive quantity the brain will suffer; compression and a series of phenomena, the symptoms of *serous apoplexy, aqueous congestion, acute hydrocephalus*, &c., supervene. This was precisely the sort of alteration the brain of my patient presented. I opened the skull by a process which is, I believe, peculiar to myself, and consists in sawing through the bones of the cranium and the substance of the brain together; the saw I employ is deeper and longer than those in ordinary use, the blade is very thin, and the teeth remarkably sharp, so that the nervous substance is not at all injured. The ventricles were enormously dilated with the accumulated cephalo-rachidian fluid, and the consequent pressure on the brain had caused the symptoms of general paralysis; the nervous tissue itself was perfectly healthy. But to what shall we ascribe this superactivity of the serous secretion? Here we have to acknowledge our complete ignorance. Should we treat such an affection by bleeding? Experience has not yet decided the question, but theory would lead us to doubt the propriety of venesection. You are aware that bleeding diminishes the proportion of fibrine and of colouring matter of the blood, increases that of the serosity, and facilitates exhalation; opening a vein, therefore, seems to me a most likely plan to aggravate the violence of the symptoms.

The animal under experiment, Gentlemen, into whose veins distilled water was injected in room of the blood extracted, presented, you will recollect, a very small proportion of serum; the emission of urine, that took place immediately after the experiment, seemed a very likely cause of the absence of serosity; to this I was inclined to ascribe it; I was in error, however. Here is the same clot I showed you at my last lecture; it is now greatly changed; it has since then undergone considerable retraction, and allowed a tolerably large proportion of serum, which was contained in its substance, to escape. Further, the clot is soft and diffluent. This shows of what slight utility the mode of examining blood usually followed really is; such examination will give no clue to the nature of the disease; nay, more, it will often prove the source of serious error, when, as in the present instance, the serosity is retained in the coagulum. For my part, I will endeavour to find out some plan capable of showing, with precision, the quantity of serum that a clot may contain. I will first try compression, and see what effect it will produce; but as to continuing the bleedings, with injection of water, in the case of the dog, of whom I was just speaking, that is impossible, for the animal died while the operator was aspiring the blood from the jugular vein by raising the piston of a syringe. I presume that air passed into the cavities of the heart; we shall learn by the autopsy if this be really the case. In the meanwhile, Gentlemen, remark that it does not follow from the fact of instantaneous coagulation taking place that the blood possesses its normal characters, for if the clot be soft it is almost certain that it contains serum interposed between its meshes. In order to fully make

out the characters of the clot we should wash it carefully, separate the globules and colouring matter from it, and so isolate the fibrine; then attentively examine the latter substance to satisfy ourselves that it does not possess the properties of what I have termed *pseudo-fibrine*. I have already explained, at some length, the points that distinguish the latter from true fibrine; I showed that it ordinarily appears, on first view, to form a considerable mass, but that its weight is much inferior to that of true fibrine; that it is less elastic and tenacious, breaking on the slightest traction. A coagulum may, therefore, be very large, and yet contain nothing but *pseudo-fibrine*, which cannot exist in blood fit for circulation. To this most important subject I shall, however, call your attention at a more advanced period of the course. There is, besides, another way of increasing the proportion of serum, namely, by injecting that liquid itself into the veins instead of distilled water. This experiment is a remarkable one; if the serum injected be from human blood the operation is followed by most serious consequences. In the animal you see on the table it has given rise to retraction of the limbs, and considerable derangement of the cerebral functions. Last year I performed the experiment also, and it caused puriform effusion into the joints. I will take care to ascertain, on the death of the animal, whether a similar phenomenon occurs in the present instance. The quantity of serum injected was ten ounces. If we introduce, instead of human serum, that of a dog, as I have done in the case of this other animal, an affection follows, attended with most acute pains, resembling those of rheumatism. The animal has grown *intactile*, if I may be allowed the expression,—to such a degree is its natural sensibility increased; the pulsations of the heart are remarkably accelerated; they are 150 in a minute. I will follow up this experiment; meanwhile I proceed to the autopsy of the animal with the soft pseudo-fibrinous clot. Just as I had anticipated, the lung is engorged at its posterior part, which is accounted for by the supine position in which the animal has laid since his death. I cut into the pericardium, which is apparently distended; no doubt there is air in the ventricles. Accordingly, on raising the wall of the right ventricle, I heard a sibilus, such as indicates the escape of gas; besides, the reddish froth, which you now see accumulated in its cavity, leaves no doubt as to the cause of death. It was evidently none other than the *accidental entry of air into the veins*. As regards the effects of bleeding the experiment must be commenced *de novo*.

SUBSTANCE OF A

LECTURE ON MEDICAL REFORM,

DELIVERED AT THE

COLLEGE OF SURGEONS IN IRELAND,

At the commencement of the Session 1838-39.

BY PROFESSOR JACOB.*

Addressing himself to the students, he asked them why they were there? What they intended by studying medicine and surgery? What was the ultimate object they had in view. Was it not to enable them to earn a respectable livelihood; to hold their proper place in society, and enjoy its comforts; to supply one of the most essential wants of the public? How was this to be done? By going forth with confidence in their strength, by seeking nothing as a favour, and claiming all due to them as a right. This, however, was only to be effected by telling the public that they were prepared to supply their wants in every particular, that they were in every respect what they pretended to be. In a word, that if a man was sick, or wounded, or required the resources of medicine, they were as competent as they were willing to administer to his necessities. Could they do this, if imperfectly educated; could they thus challenge the public if inadequately armed; could they, in a word, if only educated in certain departments of the profession, claim to practise the whole? To come to the point, could any man, educating himself as a *pure* physician, say that he supplied in his person the practitioner the public demanded? Could any man, going forth as a *pure* surgeon, make a similar claim? Had we, or had we not, arrived at the period when this absurd, mischievous, unmeaning, monstrous division of the profession into two departments was to be put an end to; to be held up to public gaze in all its deformity; to be scouted as one of those nuisances which have been begotten in darkness and nursed in selfishness. Were we, at this hour of the day, to be guided by regulations made to suit the physicians of Henry or the surgeons of Charles?

But, he continued, these are words, the expression of feelings which may be founded in error; come to facts and arguments. Suppose your *pure* physician settled down in a remote part of the country, proud of his academic honours, besotted by some idiot instructor into the delusion that a knowledge of surgery was beneath his medical dignity. Suppose him dragged by an agonised parent to see a child who has slipped with a knife in his hand and divided a large artery. The sufferer lies stretched in a pool of blood, the cloths vainly applied to stem the stream are raised, the crimson current flows, and

* We are indebted to the Editor of the "Protestant Guardian," a Dublin newspaper, for the report of this lecture.

life ebbs, while the trembling, but unworthy disciple of Hippocrates, stands a useless spectator, informing those who relied on his former professions, that, forsooth, he is a *pure* physician, he knows nothing of the art of surgery, he does not practice in that line, and so on. The scene closes. The child is a corpse, and what killed him? Not the knife on which he slipped, but the pure doctor who, standing in the way, prevented him who could have saved the patient, from approaching it. Take another case: a man, the father of a family, is attacked in the night with inflammation of the larynx; the doctor comes, he bleeds, he nauseates, he physics, he blisters, but all in vain, the mucous membrane rises, the passage narrows, the patient struggles for breath; the friends turn to the doctor; he, perhaps reluctantly, at length announces, "my art is unavailing, send for the surgeon!" But where is he? Ten miles off; not at home; elsewhere engaged. Is it necessary to follow this scene to its conclusion? The same inference follows as from the other. The life is lost because the physician was not also a surgeon. How many other examples of similar nature might I adduce? The compound fracture, the strangulated hernia, the fractured skull, the bleeding nose, the morsel in the pharynx, the dislocated limb. The case is proved; I need not go any farther. But, he continued, let me not be mistaken, I am not reflecting on physicians invidiously, or endeavouring to lower them in public estimation. The *pure* surgeon still more deserves severe animadversions. Suppose a pure surgeon settled down in a similarly remote district, educated by some college which asked him not a question on a medical subject, because they would not offend their dear friends of the College of Physicians; not a word about pharmacy, for fear their very kind and useful assistants, the apothecaries, should not be pleased; not a word respecting midwifery, because they knew nothing of the subject. Suppose this *beau ideal* of pure surgery called, for want of better, to see a case of pneumonia, of peritonitis, of hydrocephalus, of dysentery, or any other alarming disease, generally called medical. Is he not in nearly as bad a predicament as the physician in the other case, or rather is not his patient in as bad a one. It is true he is not so entirely helpless. If he has learned the principles of surgery, he has learned the principles of medicine, but if the patient's life be lost in consequence of his ignorance, he is of course as guilty as the other. Fortunately these pure surgeons are rare, we know no such abortions in our College. They are manufactured by an English house solely for the Irish market.

But, he continued, are we to stop here? Is a man educated in the two departments called medicine and surgery, fully competent to embark in general practice, or to take charge of a public situation; has he anything else to learn, any other duty to discharge not necessarily included under the two preceding heads? Has he been instructed in midwifery or obtained a qualification to practise that branch of surgery? Probably not. Persons going to the profession have assumed that they are at liberty to omit, or prosecute this part of their education as may best suit their objects in life; but should this be permitted? Should any man be allowed to take his place as a general practitioner without so essential a requisite? In whatever way that question is answered, it must be admitted, that without this qualification he should not be allowed to fill any situation in hospital, dispensary, or poor house. It was unnecessary to point out how a practitioner might be placed in the same dilemma as in the preceding instances, and by his want of knowledge be the cause of loss of life; how cases still more sudden and pressing, might demand instant attention from him; and how certainly he, and he only, should be held responsible for the consequences.

Suppose, again, the practitioner qualified in these three departments, medicine, surgery, and midwifery, is there still any deficiency; has anything been still neglected which is essential? A knowledge of the nature, properties, qualities, powers and uses of medicinal substances one would suppose would be second to no other in medical science. It is the means by which the practitioner brings all his other information into practical operation. They are the weapons or instruments by which he is enabled to control the derangements of the animal frame. No man pretending to be a surgeon is without his lancet, his scalpel, his saws, his trephines, his tenaculums, his forceps, his bistouries, his tourniquets, and so on. Should he be without his purgatives, his emetics, his sudorifics, his diuretics, his narcotics, and astringents? What is all his knowledge worth if he has not his mercurial, his antimonial, his opiate, his ipecacuan, his squill and his digitalis, on the very spot where he requires them? He is suddenly called to see a man who has taken poison—arsenic, corrosive sublimate, opium, oxalic acid, or any other equally destructive and rapid in its operation. Is he, as the pure doctor in the preceding case, to see the patient die before him, because he has neglected to provide the necessary antidotes? Will the friends, the relatives, or acquaintances, be satisfied by his excuse that he does not keep medicines, that he is no apothecary? He is called up in the night to some pressing case; he knows nothing of its nature till he sees it. It is croup, violent pneumonia, hæmoptysis, gout of some vital organ, delirium tremens in the last stage; where is the patient while a messenger goes five or six miles off for the tartar emetic, the laudanum, or the ammonia,

or while he waits till the *doctor* who keeps the next apothecary's shop returns from seeing his patients? In another point of view it becomes still more the duty of the surgeon to attend to, prepare, and mix with his own hands, the medicines he administers. To prescribe prussic acid, strychnine, the more powerful preparations of opium, or any other highly active medicines, liable to vary in strength, and at the same time to leave that prescription in the hands of ignorant persons, to take its chance among the shops of doctoring apothecaries, is, perhaps, as deliberately wicked and culpable an act as any man can be guilty of. You order a patient one, two, or three drops of prussic acid; he continues to take it; no decided effect is produced; you increase the dose; all goes on well for a day or two, but by some chance or other the prescription goes to another *doctor's* shop; a new bottle is opened of double or treble the strength; the same number of drops are put into the draught, the patient swallows it and dies. Now, who in this case has poisoned the man? It is not clearly he who used so powerful a remedy, without those precautions which he should entrust to none but those in whom he reposed the most unlimited confidence. Of all the causes which have retarded the progress of medical improvement, and diminished the utility of medical men, the infatuated prejudices about compounding and administering medicines are the greatest. It is said that to provide medicines for one's own patient is degrading, beneath the dignity of a professional man, making a profession a trade. This is the argument of men who live in palaces and ride in coaches, who neither can or will look beyond the circumscribed horizon of their limited vision: it is utterly inapplicable to the case of the hundreds to whom I address myself. If you will not accept the comfort and independence of the English general practitioner, with the rank he enjoys in society, then like true born Irishmen starve in your dignity and welcome.

Having endeavoured to point out the course which your interest and duty indicates, the next step is to inquire how far it is in the student's power to pursue it. What has the Legislature done for him? The answer must be, nothing, or worse than nothing. There is a strange feeling in England with respect to medical matters. They seem to consider that it is part of Magna Charta, or the Bill of Rights, that every man should be poisoned or mutilated if he pleases; that whoever pleases should practise medicine, and every man judge for himself as to the qualifications of the parties. This may do very well for the survivors, but very ill for those who perish in the progress of the experiment.

But direct encouragement and protection are afforded to quacks by the Legislature; which, instead of discouraging the sale of poisons under the name of patent medicines, encourages it for the income it affords through the Stamp Office. What cares the Chancellor of the Exchequer for coroners' inquests and manslaughter convictions, if the drastic pill or concealed arsenic figures conspicuously in the quarter's account. In Ireland, thanks to our poverty, or the shrewdness of the people, or their ready appreciation of the ridiculous, the evil is not so great. Quackery, except under the authority of a sham diploma, or an apothecary's certificate, is an unprofitable calling. We have ladies bountiful and gentlemen bountiful, who, as long as they keep from cancer plasters and such dangerous matters, deserve commendation, not reproach, but who should, on all occasions, be warned not to meddle with edged tools.

In England even the public institutions are unprotected. As far as I can find there is no law to prevent hospital or dispensary governors or poor-law officials from appointing any creature who calls himself *doctor* to these institutions. It is otherwise in Ireland. Fifty years ago, when the county infirmaries were first established, a provision was made for securing the services of properly-educated surgeons for them. This provision still continues in force. Last year a desperate attempt was made to assimilate the law in Ireland to the lawless practice in England, and to enable the holder of a diploma obtained on a twelve months' study, and twenty minutes examination, to fill the most important medical situations. This nefarious attempt was defeated by this College at an expense of a thousand pounds. We have been accused of retaining a monopoly by thus defending the exclusive right of our licentiates to be appointed to the infirmaries. But we have, over and over, and over again, offered to surrender the privilege on condition that a proper qualification should be insisted on. We are not so unfair or selfish as to deny that there are many men in Ireland as well qualified as our licentiates for these situations; but we also know that there are many holding diplomas utterly unfit to fill them. All we ask is a provision to enable and compel governors of hospitals to draw the distinction—to separate the grain from the chaff.

The next question for the student is how this education is to be attained. Is the nonsensical plan of going to one college for one part, and to another for a second, and somewhere else for a third, to continue? Is the student still to be compelled to go first to some institution where he expects to finish his education, and after some years to be told that he must go elsewhere to make him perfect? One says "I will make you a pure doctor;" another, "I will make you a pure surgeon;" another, "come to me for a midwifery diploma;" and a fourth, "here's a licence to sell drugs;" making you not what you should be, a useful apothecary, but a

most respectable Jack of all trades and master of none. Is not the remedy for all this mischief and confusion obvious? Let institutions for educating general practitioners in all departments be established, or existing ones encouraged. It is true that few of the present medical corporations can educate or licence the general practitioner, neither do they wish to do so. The Universities would not descend from their high places; and if they would they could not, reverencing as they do the injunctions of Henry, Elizabeth, James, and Charles. The colleges of pure surgery are equally disabled. The impudent pretensions of the retail druggists or apothecaries to meddle in any way with medical education, is, of course, only to be treated with contempt and derision.

The licentiates of the College of Physicians, a body unauthorised to grant what are called medical degrees, assume the title of doctors. Every man who gives physic is to the public a doctor. But is not the magic of the title gone for ever? Who would call himself doctor for distinction sake, or to establish his professional superiority? Should not a man at present pause before he fixes indelibly a title on himself which he must enjoy in common with the very lowest grade of pretenders to the healing art? Where is the honour in the title which an apothecary can obtain by stepping from behind his counter into a Scotch steam-packet in November, and returning next day by the same conveyance, and repeating the same migration in the following May? No, that delusion has passed: the public must have some better proof of your capacity than that "leather or prunello."

[The remaining portions of the lecture related exclusively to arrangements and misarrangements in the Irish medical corporations and schools, and being of less general interest to the profession, we abstain from its republication.]

ON THE IMMEDIATE CAUSE OF

DEATH FROM DROWNING,

AND THE

TREATMENT OF SUSPENDED ANIMATION.

By MALCOLM W. HILLES, *Esq., Surgeon, Westminster.*

THE immediate cause of death from drowning, or immersion under water, is not generally known, and is, therefore, frequently misunderstood; the treatment, too, I am sorry to say, adopted generally in such cases, is quite inapplicable.

Many suppose that death is produced by the water entering into the trachea, which thus filling the air-cells of the lungs, produces suffocation, from the exclusion of the atmospheric air. So far from this being the case, it is found, on making a post-mortem examination, that not even a drop of water has entered the trachea. How, then, is suffocation induced? Simply as follows:—As soon as the individual becomes immersed beneath the surface of the water, an attempt (frequently an involuntary one) is made to inspire; the water, by this means, is drawn into the larynx, but the first drop which touches the chordæ vocales, throws the constrictor muscles of the larynx into spasmodic contraction; the rima glottidis is immediately closed, and is retained so until all attempts at respiration cease, and even for some hours after. This, then, is the first function suspended, but circulation also soon ceases, and death results.

That such is the state of parts I had an opportunity of verifying in a subject brought into the Westminster Hospital School, a few weeks since, and which had been taken out of one of the canals in the neighbourhood of London. In this instance the lungs did not contain any water; they were, however, gorged with blood, particularly towards their posterior surface (caused, no doubt, by the gravitation of the fluid to this part, from the position of the subject), and a quantity of frothy mucus was contained in the interior of the trachea. On opening the heart its cavities were completely emptied of blood, with the exception of a small quantity, which was still fluid, in the right auricle and ventricle.

A cessation of respiration, caused by the spasmodic closing of the rima glottidis, being, then, the cause of death, the most judicious plan of treatment is evidently to restore this function as soon as possible. This cannot be effected so long as the spasmodic action of the muscles continues; we should, therefore, at once proceed to the introduction of air by mechanical means.

Two plans have been recommended for this purpose; one, the passing of a catheter through the nares into the larynx, by means of which, and a complicated bellows attached to its free extremity, the air may be injected into the lungs, and again removed; the other, the performance of the operation of laryngotomy. The first of these plans is highly objectionable, as few are provided with the instruments at hand, and they, even if convenient, are so difficult of application, that much time must necessarily be sacrificed before the surgeon can assure himself that he has passed the tube into the trachea, and not into the œsophagus. This he can only ascertain by introducing his fingers into his patient's mouth; but how much valuable time is lost in doing this? A minute; nay, a few seconds, may decide the life of the patient, and the surgeon sacrifices many, in groping for the extremity of the catheter in the fauces of his victim! But at length all is set to rights; the operator commences the process of inflation; the air-cells are forcibly distended with air;

they give way, and an emphysematous lung soon disposes of the small chance that had previously existed of the patient's recovery.

The operation of laryngotomy is liable to none of these objections; it is performed in half a minute, or less, by passing a trocar and canula into the larynx, through the crico-thyroid ligament, or, when these can not be procured, with a common lancet, or even a penknife, a quill or strong straw being made to answer the purpose of a canula; the air being caused to enter and escape from the lungs, by alternately compressing the lower part of the thorax, and then removing the pressure, thus similating, as far as is possible, the natural act of respiration. Some, perhaps, may object to the operation on account of the difficulty of its performance, and the wound which is made into the larynx; but both these objections are futile, and can never be entertained for a moment by any person at all acquainted with the subject. The operation is more simple than that of bleeding, and the wound, if closed with adhesive plaster, will heal without any bad consequences.

While the surgeon is engaged in performing the operation, the application of heat to the surface of the body, and stimulant applications to the integuments of the chest, and to the mucous membrane of the nose, should be resorted to. Of these the liquor ammoniæ is the best, as most rapid in its action, care being taken to prevent its escape by evaporation, when applied to the integuments of the chest. I see no objection to the spirits of wine being applied over the thorax, and then set on fire; we may, by so doing, rouse the action of the heart and muscles of respiration, which, if once procured, success is almost certain.

If we contrast the phenomena just mentioned, which occur during drowning, with those that present themselves in the production of asphyxia from other causes, such as the inhalation of carbonic or other gases destructive to life, we shall have much reason to *expect* a more favourable result to our efforts in the former than in the latter cases. In the one, a destructive gas has been slowly taken into the system, venous or imperfectly oxygenated blood has circulated in the brain, heart, and arteries; the nervous system has been slowly paralysed, and its energy destroyed, by the direct contact of a noxious agent; in addition, the muscular fibres of the heart, vessels, and muscles of respiration, have been deprived of contractility by the same means, either directly by the action of the gas on the muscular fibre, or indirectly through the nerves distributed to these parts. All these effects must be superseded before we can indulge in the hope of our patient's recovery; but not so in asphyxia from drowning; here, there is no destructive agent to contend against, no deleterious principle to be removed, no noxious vapour to be neutralised; suspended animation has been caused by the *want* of the proper stimulus, namely, the atmospheric air, not by the *presence* of any ingredient injurious to life; nor have we, here, the circulation of the venous blood in the arteries of the brain, as the action of the heart ceases too rapidly for this to take place.

But how different *has been* the result! How often have individuals been recovered from a state of suspended animation, caused by their immersion for many minutes in a deleterious gas; how seldom after drowning?

It is true that in drowning the immersion of the body in the water, particularly in cold weather, lessens our chances of success, the animal heat being so much more rapidly carried off or absorbed by this fluid, than by any of the gases I have mentioned; this, however, can only have such influence during the winter months.

I cannot too highly deprecate the practice of a number of persons crowding into the room, when the efforts are being made to restore suspended animation; indeed, such is the state of the air frequently in those apartments, that it is better suited to the suffocation of the living than to the kindling of the feeble spark that may still animate the prostrate figure before us.

The operation of laryngotomy should also be performed in all those cases of sudden death which occur during meals, if we have reason to suspect that a morsel of food has become impeded either in the pharynx or larynx. In the former case the judicious surgeon will first attempt the removal of the foreign body with his fingers or the probang.

If a body have remained for twelve or twenty-four hours in the water, we may expect to find some of the fluid in the lungs, as by this time the contraction of the muscles of the larynx will have ceased.

7, Duke-street, Westminster,
Nov. 19, 1838.

ON THE
CAUSES, NATURE, AND PREVENTION
OF
ICTERIC OR YELLOW FEVER.

By Francis Eagle, *Esq., Surgeon, London.*

Yellow fever is an effect almost peculiar to the tropical zones (*Blane*); it is not entirely so, for it is not uncommon in the southern part of North America, and has been even seen rarely in France and Spain, but constantly preceded in these latter countries by unusually hot weather. Now, if there be a peculiar effect, there must also be a peculiar cause; and what is that peculiar cause but elevated temperature? For nei-

ther rank vegetation, nor marshy ground, nor excess of moisture, nor irregular elevation of soil, are *peculiar* to the tropical zones, since they are also *common* to Great Britain, France, Ireland, Germany, and other countries, where yellow fever is never seen, or seen only in the form of jaundice. What is the action of elevated temperature on the body? Dr. Johnson says "increase of bile and increase of perspiration." Elevated temperature is, then, a first and necessary cause of yellow fever, a "cause continent" (*Bacon*) of this disease; but other causes may contribute to this effect, and especially such causes as act in a similar manner (*Mosely*), such as excess in eating or drinking.

It has been objected (*Craigie*) that elevated temperature cannot be the cause of yellow fever, because the disease does not constantly occur at the maximum of temperature, but that it occasionally rages during the cold season of the year (tropics); this objection is more specious than solid, for a variation of five or six degrees, in a mean temperature of 79° or 80°, still leaves a temperature amply sufficient for its production; and a body of troops, arriving at the West Indies from Great Britain, even in the best seasons (*Mosely*), would still be liable to yellow fever; this liability will be less, first, and principally, in a direct ratio with the lowness of temperature; and, secondly, according to the diet adopted; the full diet of red-blooded animals, and highly-stimulating drinks, being incompatible with a high degree of heat; the cause of which physical philosophy will probably ere long unfold. Hence, even in Great Britain, we approximate, in a degree, to the diet of the native of the tropical zones, when the temperature undergoes a similar approximation (fish in summer). At a temperature of 50° Fah. fresh cases of yellow fever soon cease to appear (*Dr. Gilkrest*).

In order to make still more plain this simple, yet I believe philosophical, view of the subject, it will be well to remark the physiological differences in the white and black varieties of the human species.

Lord Bacon observes, — "There is no doubt but that the figure and structure of the inward parts is very little inferior, for variety and lineaments, to the outward members; *and that hearts, and livers, and ventricles, are as different in men, as either their foreheads, or noses, or ears.*" "And in these differences of inward parts, there are often found the *causes continent* of many diseases, which physicians not observing, do some time accuse the humours which are not delinquent, the fault being in the very mechanic frame of some part."

The peculiarities in the anatomical formation of the white and the negro, are very material, and exercise great influence over their susceptibility to disease. In the osseous system, the shape of the skull and face, the spine, the ribs, pelvis, and extremities differ much; the internal viscera, also, are equally peculiar, especially, perhaps, the liver, and externally the skin, hair, &c.; but this part of anatomy, viz., human comparative anatomy, has been so imperfectly studied, that in the extensive and excellent library at the College of Surgeons, I find only one work (*Soemmering*) on this department, while those on general comparative anatomy abound.

This is the more extraordinary, because, as remarked above by Bacon, human comparative anatomy embraces the causes continent of many diseases; nay, does it not constitute the foundation of that "true and natural system of philosophy upon which the science of medicine must yet be built; for medicine not grounded on philosophy is a weak thing!"—(*Bacon.*) But not only is their anatomical condition peculiar, but also their physical condition generally, especially as regards diet, &c.; and strange, indeed, it is, that sixty centuries have not taught man the plain philosophy "to do at Rome as Romans do." The change called "seasoning" which Europeans, and the inhabitants of the temperate zones generally, undergo, when removed into tropical climates, is nothing more than an approximation, however distant (*Mosely*), towards the physical condition of the native, which change is peculiarly clear and prominent in the hepatic and cutaneous organisations; or, to borrow "a term" the cutaneo-hepatic sympathy (*Dr. Johnson*). Here, then, lies the second cause of yellow fever, the persistance in European diet (and even a greater degree of stimulation as regards drink) in tropical climates, and the following order of mortality illustrates this view:—

Statistical Reports of Captain Tulloch.

NATIONS—
- English (maximum);
- French;
- Spaniards;
- Africans;
- Natives (minimum).

INDIVIDUALS—
- Officers (maximum);
- Soldiers;
- Women;
- Children (minimum).

Thus, all the above suffer in a direct ratio with their high diet, and it is further advanced by veteran authority (*Mosely*), that by temperance and abstemiousness the noxious influence of tropical climates may be rendered almost nugatory; and yet, strange as it may appear, the troops serving on these stations, receive a double amount of pay, a premium, as it were, for mortality and death.

It may, however, be objected to this theory, that the above nations suffer in pro-

portion to their emigration from a more northerly climate; and it is confessed that the coincidence is worthy notice; but it is *at once negatived* by the broad and indisputable fact, that even amongst the same nation, *cæteris paribus*, the most abstemious enjoy the greatest immunity from yellow fever. It is remarkable that influence of diet is scarcely hinted at by many learned authors on this disease (*Craigie, Gilkrest*), and even in the Statistical Report is almost entirely omitted; still a glimmering of philosophy breaks out in the following remarks on the rations and diet in the Bahamas command.

"The weekly ration of the white troops in this command has generally consisted of 7lbs. of bread, 5lbs. of salt meat, 2lbs. of fresh meat, *and a quart of rum*. That of the black troops has been varied according to the state of their health, *it having been found that they suffered more from a salt meat diet than the white*." The following is the weekly ration established for them in 1828, and which is understood to have continued with little alteration since that period:—7lbs. of bread, 1½lbs. of salt meat, 1½lbs. of salt pork, 10 oz. rice, 2½ pints of peas, 9 oz. of sugar, 5 oz. of cocoa, 3½ gills of rum, but the relative quantity of fresh and salt meat, has varied according to circumstances. To the peculiar condition, and the peculiar physical circumstances, are to be attributed all the *peculiar effects* observed in the tropical, and, indeed, every other climate.

Herein lie the why and the wherefore of the white troops suffering from yellow fever, the black from diseases of the lungs, bowels, and brain, and almost exclusively from cholera; why *both* suffer from ulcers; why Tobago is remarkable for fever; Dominica for diseases of the brain and bowels; Barbadoes for those of the lungs; Grenada for those of the liver; Trinidad for its dropsies; they all deviate just so much as their physical causes deviate; they approximate and are identical just so far as their physical causes approximate. Here is the shadow, but where is the substance? Here is the *outline*, but where is the *body*?

From the preceding it appears that the result of the researches of the Baron Humboldt, on the subject of yellow fever, that the disease never occurs beyond an elevation of 2500 feet, is *illusory*, a false induction from true and indisputable facts, since elevated height implies *diminution of temperature*; and this latter is the true cause of immunity from yellow fever.

If, then, I have been able to draw one logical induction from the facts contained in the Statistical Report, *one positive* as opposed to the *nine negatives* of that report, I arrogate to myself no further credit than that of a simple observer of Nature, according to the philosophy of the immortal Bacon. "For men," says he, "use commonly to take a prospect of Nature, as from a high turret, and to view her afar off; and are too much taken up with *generalities;* whereas, if they would *vouchsafe to descend*, and approach nearer to particulars, and more exactly and considerately look into things themselves, there might be made a more true and profitable discovery and comprehension. Now, the remedy of this error is not alone this, to quicken or strengthen the organ, but withal *to go nearer the object*."

The pathology of yellow fever is not more easy of solution, than that of either of the before-mentioned diseases; and, as in the medical department of philosophy, the advancement of truth will greatly diminish, if not thoroughly extinguish disease, by the exercise of our knowledge over the *causes incontinent* or exciting of disease, so in the moral department, as was long ago observed by Plato, and echoed by Bacon,—"Then will nations be happy when their kings are philosophers, or their philosophers kings."

COMPRESSION IN CHRONIC HYDROCEPHALUS.

To the Editor of THE LANCET.

SIR:—In your retrospective review of the state of medicine for 1838 (LANCET, September 22, p. 61), in speaking of Dr. Englemann's cases of chronic hydrocephalus, treated so successfully by pressure, you have mentioned my name, not exactly in a way which I think I had a right to expect. You have put my originality on my own *ipse dixit*, and left it entirely to the public to make what interpretation they choose of the ambiguous phrase:—"Mr. Barnard assures us that the plan originated with himself." This is certainly not what I would call justice, after I had referred you to my cases, published fifteen years ago in the "Medical Repository," of which you cannot plead ignorance, for one was copied by you into the first volume of THE LANCET, therefore there can be no want of proof Independently of all this, Dr. Englemann himself acknowledges that he took the plan from me.*

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* We regret that Mr. Barnard should think that we were not inclined to do him "justice" in our allusion to the proposal of compression as a means of cure in chronic hydrocephalus. The following remarks will justify, we trust, even to the satisfaction of Mr. Barnard, the guarded opinion which we gave on the "discovery" alluded to in his letter. In the "Observationes Medicæ" of Riverius, published in 1656, p. 327, we find the following case:—"A new-born infant laboured under hydrocephalus, with *great dilatation of the sutures*.

You have said that I am to blame in not publishing the cases which have come under my care since the first two, but they resemble each other so nearly, that it would be almost a repetition of the same words. Some, of course, have required more time; some are more severe; but in all the symptoms so closely resemble each other, and the treatment has had so decidedly the same effect, that I considered it quite unnecessary to take up any room in your valuable periodical, or the time of the profession in reading, until I had a mass of evidence upon which others might rely as well as myself. However, I select one case out of six; it presents no peculiarity, and differs in no respect from the others, excepting, perhaps, in the time employed in the cure, and the severity of the symptoms.

January, 1832. Jane Parfitt, a child, eight months old, was born with a large head, which has sensibly increased up to the present time. Both the anterior and posterior fontanelles are very open, and the parietes distended; the bones of the skull are thin, moveable, and separated from each other; is constantly in a recumbent position, from inability to sustain the head upright. Pupils dilated, and insensible to light; slight strabismus, occasional convulsions, and great restlessness; sickness, and unnatural secretions from the bowels; appetite good, almost voracious; face pale and emaciated; an inattention to surrounding objects, amounting almost to coma.

Although the case appeared to be almost hopeless, I determined on trying the effect of pressure. Accordingly I directed the head to be shaved, and on the 15th of January I applied the adhesive plaster, first, in long strips, completely round the head, as low down as the ears would permit, sufficiently tight to give complete support to the loose and moveable bones; then, with cross strips, over the top, from side to side, and, lastly, one long strip, reaching from the forehead, within half an inch of the nose, to the nape of the neck. The circumference of the head is nineteen inches. The compression produced no additional uneasiness, nor any increase of symptoms. Castor oil to be given occasionally if required.

During the first month there was little apparent alteration in the size of the head, or the state of the patient, excepting that the convulsions were thought to be not so frequent, and the evacuations had a more healthy appearance. The castor oil had only been given twice; strapping quite firm.

March 2. Has had no convulsions for a fortnight; strabismus gone; evacuations natural and regular; head appears to be gradually getting firmer; expression of the face much more pleasant and healthy; sleeps well.

15. The plaster was renewed, as it had become corrugated in several places; the long strip from the forehead to the occiput was omitted, as, from the increased firmness of the head, it appeared to be useless. At this renewal the head was found to have decreased in size half an inch.

The case went on well for six weeks, without one untoward symptom; the child has increased in flesh; the evacuations are natural; the convulsions and restlessness have entirely disappeared; the head is much firmer; the fontanelles are smaller, and the sutures are nearly closed.

May 3. Strapping renewed; the head, by measurement, has diminished but little; the child is better able to move it, although the muscles of the neck have not yet sufficient strength to sustain it; has cut four teeth since the commencement of the treatment.

From this time the little patient gradually improved in health and strength. The plasters were again renewed, for the last time, about the latter end of July; the child was now able to hold up its head with but little oscillation. In September it appeared in perfect health, with a head much larger than common, measuring about eighteen inches in circumference, but quite firm in every part, excepting a very small portion of the anterior fontanelle. I saw the child twelve months after, when it seemed in every respect to be well. At this time the parents left Bath, and I have not seen it since.

I cured the affection by simply surrounding the whole head with Guidon's bandage. The bandage was renewed every day, and the tumour had disappeared after the lapse of from 15 to 20 days."

Rosen's work on Diseases of Children, a treatise familiar to every practitioner, which was translated into French in the year 1778, contains the following remarkable passage, p. 503:—"At the same time the child's head should be submitted to *gradual compression*, in order to bring together the cranial bones, in proportion as the fluid is discharged (by purgatives, blisters, &c.). This compression may be made with linen dipped in alcohol, &c., or with a cap or circular band, constructed for the purpose.

Finally, in the "London Medical and Physical Journal," October, 1821, we find a paper by Sir Gilbert Blane, entitled, "On the Effects of Mechanical Compression of the Head, as a Preventive and Cure in certain Cases of Hydrocephalus." Sir G. Blane describes two cases of chronic hydrocephalus, successfully treated by gradual compression, and alludes to several others, which were treated in a like manner by his friends.

The case of Mr. Barnard was published in the "London Medical Repository," on the 23rd July, 1823.—Ed. L.

This has been the usual course of my cases, of which a repetition would be useless. Perhaps the number would be considered to be too small to establish any particular mode of treatment; for, although I have been hitherto uniformly successful, I do not expect that in increasing my number I shall or can, in every instance, secure the same result. Yet, my six cases, with Dr. Englemann's ten, form no despicable number in relation to a disease which is comparatively rare, and has hitherto been fatal, on which to build hopes of at least a pleasurable kind. I am, Sir, yours, &c.

J. F. BARNARD.

Bath, Nov. 19, 1838.

THE LANCET.

London, Saturday, December 1, 1838.

THE adulteration of drugs is probably carried to a greater extent in England than in any other country. Pharmacy is at the lowest ebb; the druggists, ignorant in too many cases of chemical science, cannot test the articles which they procure from the wholesale dealer, and are thus either driven to pay the Apothecaries' Company exorbitant prices, or to mix adulterations and abominable compounds, which often do the patient no good, and sometimes do him incalculable mischief. The evidence which Dr. THOMSON gave before the Poor-Law Committee threw much light upon the art and mystery of adulteration, and we are glad to find that the subject is beginning to excite interest in many quarters where the light never comes, or comes late,—we mean the hospitals scattered over the kingdom.

The County Infirmaries, like all other public institutions, are exposed to jobbing tradesmen of every description, who, on the force of a two-guinea subscription, think themselves fairly entitled to extract twenty times the sum from the funds of the institution, and from the sick-poor. The officers shut their eyes, or sleep at their posts; and while the spirit of jobbing is rife the honest dealer is infected, or, if he preserve his integrity, and supply the institution with articles, which, in his conscience, he believes genuine, he is exposed to violent and, perhaps, unjust suspicions.

These reflections have been suggested by a stormy meeting of the Trustees of the Salop Infirmary, reported in the "Shrewsbury Chronicle." We shall allow one of the physicians to introduce the matter in dispute:—

The Annual Report having been read, Dr. J. P. JOHNSON called the attention of the meeting to the propriety and expediency of purchasing the drugs, &c., used in the Infirmary, from London houses, instead of having them from chemists in that town, by which a considerable sum would be saved, and purer articles obtained. He said, this plan had been followed, with great advantage, at the Stafford Infirmary and Birmingham Dispensary; and added, that it was extravagant in the directors to pay a dispenser when he saw such things as blue-pill charged in the chemist's bill. He should recommend that the drugs be purchased from London, and that a dispenser capable of preparing orders should be engaged. By this method upwards of 100*l.* was saved at Stafford Infirmary last year; and was that sum to be given away in this town? It was making a Tommy-shop of the institution. They tied down their baker, butcher, and maltster to the lowest penny [*Query?*]; and were they to favour others who sent in inferior articles? *As a specimen, here was a compound of rubbish which had been sent in as bark, when, in fact, it was nothing but* WILLOW AND ELM BARK, *and other stuff.*

Mr. T BLUNT—We have served this institution to the best of our ability ever since 1809, and I will ask Dr. Johnson if he can say we ever sent bad drugs?

Dr. JOHNSON—No, I can't say you ever did; but you charge very high. The bad bark was sent by Mr. Poole. There's not a respectable man in town but what sends to London for his drugs. (*Disapprobation.*)

We do not see why the directors of County Infirmaries should purchase drugs and medical appliances exclusively from London houses. If the articles supplied by the operative chemists of the place are as *good, and as cheap*, as a London house would furnish, why should the home-trader be overlooked? On the other hand, nothing can be a more flagrant sacrifice of the interests of the institution than the 94th statute of the Salop Infirmary. It is there enacted, that "the drugs supplied to the institution shall "be purchased *only* from operative chemists

"in the town." If the operative chemists keep good and cheap drugs this regulation is unnecessary; and it is a direct bonus on inferior articles and extravagant prices, because it tells the operative chemists of a small town that they shall supply the Infirmary, on their own terms, with whatever adulterations they please. It completely shuts out the wholesome principle of competition, and deprives the Infirmary of all the advantages possessed by private individuals. What private practitioner ever *bound* himself to deal invariably with the same druggist. Mr. POOLE is a respectable tradesman, and would, no doubt, have withstood the temptations offered by the statute if they had not been of an extraordinary nature. But, under such an unwise regulation, no one could be surprised to find him often sending "willow and elm bark, and other rubbish," in place of pure cinchona. Such a result could scarcely have been contemplated by the framers of the law; but its direct effect was to make the chemists negligent and to make them keep bad drugs; to enrich the trader, whoever he might be, at the risk of cheating the sick-poor of health and life.

The genuineness of the remedies employed at the County Infirmaries is an indispensable condition of their usefulness. The charitable of all classes subscribe thousands of pounds annually, the physicians and surgeons give their services, but the money and the skill are all thrown to the four winds of Heaven, when the chemists, to gain 50*l.* or 100*l.* annually, supply inert substances instead of remedies. In the present state of pharmacy these Infirmaries can only secure themselves against fraud by grinding all their own powders, by making their own tinctures and extracts, and by employing, as the London drug-brokers do, analytical chemists. Let the supplies of drugs be procured every half-year, and four or five articles, selected for the purpose from every lot, be forwarded to an analytical chemist, and the institutions need not dread adulteration; they can then study economy, and reduce their drug bills some 50 per cent.

The debate opened by Dr. JOHNSON, ran into a sort of Hudibrastic conflict, and led to no decision. Mr. WHITNEY reminded Dr. JOHNSON that he had declared *nitric* ether was made with *sulphuric* acid.—Dr. JOHNSON explained the mistake amidst laughter and noise. Mr. T. BLUNT had no doubt that little was cared for Dr. JOHNSON. Dr. JOHNSON challenged Mr. T. BLUNT or Mr. WHITNEY to equal the *plaster* which Mr. RICE WYNNE procured from London. Mr. WHITNEY cleverly demonstrated, by the strict doctrines of probability, that "the sample of bark, which had been sent by Mr. POOLE, had by *accident got mixed in the drawer with other barks.*" This completely exculpated Mr. POOLE. Dr. DU GARD bore testimony to the excellent quality of the drugs. He was quite sure that no chemist could render *his* prescriptions more efficient than the calumniated Mr. POOLE. Another subject was broached.

A letter was read from Mr. SUTTON, senior surgeon to the Infirmary, announcing his resignation. A vote of thanks to Mr. SUTTON was proposed by Dr. DU GARD. Dr. JOHNSON could not agree to the motion. He was compelled to move a vote of censure on Mr. SUTTON: Mr. SUTTON acted wrong in August last. Mr. G. WOOD could say, that although Dr. JOHNSON was now so careful of the Infirmary not being injured by its other officers, he attended only *once in six months* himself, when he (Mr. W.) was a dresser.—Dr. JOHNSON replied. Dr. DU GARD was about to bear testimony to the valuable services of his colleague, when Dr. JOHNSON (who was evidently *highly excited* by the storm of epithets and groans showered upon him) exclaimed,—"Oh! it's very fine for Dr. DU GARD to talk, when he is not a regular physician himself." (The Doctor has a St. Andrew's Degree!) This was the signal for renewed uproar, which lasted several minutes, after which Dr. DU GARD said, "Regular or not regular, I did your business in this house upwards of nine years." Dr. JOHNSON denied that. Dr. DU GARD, who apparently knows less of round robins than

he does of physic, modestly added, "I, for *one*, would certainly put my name to a *round robin*, never to act with Dr. JOHNSON." Dr. JOHNSON is a formidable rival: every one applauded Dr. DU GARD's disinterested ardour. Mr. POOLE was in transports of admiration.

Mr. SUTTON had performed his duties to the entire satisfaction of the sick-poor and the directors; the vote of thanks was carried by acclamation. He has always treated his pupils with the greatest liberality and kindness; but it was on the score of having taken a pupil that Dr. JOHNSON's charge rested. A certain chemist by profession rather than instruction, has imposed for many years upon the old women of Shrewsbury; and not only vended his nostrums over the counter, but visited patients extensively at their own homes. By dint of destructive experience that foolish person began to perceive the mischief he was doing, and several cases of manslaughter accumulating on his hands, disturbed his fears, and made him uneasy lest he should be called to meet the injured patients who had escaped with their lives, in a court of justice. He entertained no apprehensions from the dead; and had seen many children, many parents, die under his hands with impunity. The hour of reckoning was drawing nigh; he determined to *study* the art which he had so long *practised*; and he became Mr. SUTTON's *pupil* at the Infirmary, still carrying on, it would appear, the trade in drugs and quackery.

The meeting, to conclude, elected Mr. KEATE to the office of surgeon, by a majority of *one*, and the decision was subsequently disputed by the other candidate; but the election was very properly confirmed, Mr. CARTWRIGHT having attended little to surgery, while Mr. KEATE has zealously prosecuted that branch of the profession.

THE ANIMAL MAGNETISM FRAUD AND HUMBUG.

Is it true that any physicians or surgeons of any one of the London Hospitals are insulting public decency, and abusing the confidence which has been reposed in them by parents and guardians, by still practising animal magnetism on the sick and deluded patients? If there be such a hospital,—if there be such officers belonging to any establishment in London,—the sooner the governors take serious notice of what is passing in their institution, the better will it be for the interests of the charity, and the reputation of the science of medicine in this country. After the exposure, the extraordinary, but most complete and effective exposure, of the folly of believing in the reality of animal magnetism,—accomplished to the satisfaction of every sane spectator, at a private residence in London,—we had thought that the humbug would no longer be tolerated in any of our public institutions. If, however, we are correctly informed as to what is passing, at least in one of them, we have been deceived in this expectation. We have a duty to discharge to the profession in this affair, and it shall be executed to its fullest extent. The nuisance shall be attacked, the humbug shall be pursued, until it is thoroughly and finally abated. Our chief object in writing this notice, is to solicit immediate information on the subject to which it relates. If there be such a hospital as the one we have described, the medical school with which it is connected must be speedily and irreparably ruined, unless the immoral quackery be at once put down by the governors, or other controlling body of the institution, in which the heinous enormity against common sense and female delicacy is perpetrated. Sincerely shall we rejoice to find that we have been misinformed by the reports in question.

We owe to the politeness of Mr. Greer, an account of the post-mortem appearances of James Trickey, which will be found in another part of this Journal. Having received Mr. Greer's communication at a late hour on Wednesday, we are compelled to defer, until next week, some observations which we had prepared on the case of James Trickey, and on the heating apparatus of Messrs. Harper and Joyce.

Nugæ Chirurgicæ; or, Biographical Miscellany. By the late Mr. Wadd. London: Longman and Co. 8vo. pp. 95.

We have room for only an extract or two from this work.

Sir Edmund King, Physician to Charles II., was originally *a surgeon*, and saved the King's life, at a critical moment, by bleeding him.

Evelyn gives the following account of his bleeding King Charles (Vol. iii., p. 580):—

"1685, Feb. 4.—I went to London, hearing his Maty. had ben, the Monday before (2 Feb.), surprised in his bedchamber with an apoplectic fit, so that if, by God's providence, Dr. King (that excellent Chirurgeon as well as Physitian) had not been accidentally present to let him blood (having his lancet in his pocket), his Maty. had certainly died that moment, which might have been of direful consequence, there being nobody else present with the King, save this Doctor and one more, as I am assur'd. It was a mark of the extraordinary dexterity, resolution, and presence of mind, in the Dr., to let him blood in the very paroxysm, without staying the coming of other Physitians, which *regularly* should have been done, and for want of which he must have a regular pardon, as they tell me."

The Privy Council approved of what he had done, and ordered him £1000, but which was never paid him.

The Medical Annual; or, British Medical Almanack, 1839. Edited by William Farr. To be continued annually. London: Sherwood. pp. 224.

When works of this description are executed with sagacity, research, and industry, they become treasures of no ordinary value to the members of our profession. The present Annual is a Pantalogia, wherein the medical practitioner may find, at a glance, many things which it is important that he should know, and with which he should at any moment be able to become immediately acquainted. In the contents of the present Annual we find a Calendar; Tables of Heat and Dryness for the last ten years; Information relating to all the chief Hospitals, Infirmaries, Medical Colleges, Boards, Schools, and Societies, the Universities, and Lunatic Asylums; a Paper by a Clergyman on the Misarrangements and Arrangements of Hospital Staffs; A History of the Medical Profession; Statistics of the Kent Hospital; A Paper on the Pathology of the Heart; one on American Temperance; one on the Mortality of Cholera; one on the Diseases of the Army; and one on Private Medical Practice.

These papers and tables exhibit many instructive facts, particularly the article relative to the Cholera, a portion of which has already appeared in the pages of The Lancet. It contains a very important view of an intensely interesting subject, and—prepared with such materials as it presents—the practitioner encounters an attack of cholera with a degree of fortitude and power which he could otherwise neither exhibit nor command.

The article entitled "History of the Medical Profession, and its Influence on the Public Health," presents evidence of great labour and research. This essay is the product of Mr. Farr himself, who, it is evident, has collected materials from every available source, and produced an article of peculiar interest and permanent instruction. We shall make from it a few extracts on another occasion, and in the mean time cordially introduce the volume to the notice and patronage of the profession. It must prove equally useful in the surgery and the library of every general practitioner in the kingdom.

BRITISH MEDICAL ASSOCIATION.

November 27th, 1838.

The minutes of the last meeting having been read and confirmed,

Jas. Stilwell, Esq., M.R.C.S., Uxbridge,

Wm. Rayner, Esq., M.R.C.S., Ditto,

were admitted members of the Association.

Mr. Wallace, of Hackney-road (an ordinary member of the Society), related a case of mala-praxis of a man-midwife (an illegal practitioner), Mr. Baker by name, and living in Mr. Wallace's neighbourhood, where

presumptive evidence appeared of the child's death having been caused by the use of instruments. Mr. Wallace stated his having called on Mr. Baker, the coroner, to summon an inquest on the body, when Mr. W. was told that no cognizance could be taken of the affair, as the child was *born dead*. Mr. Wallace further mentioned two other cases where coroners' inquests had not been held upon the bodies, when, from circumstances, medical evidence might have thrown great light upon the causes of death.

The following communication was then read from Martin Sinclair, Esq. of Manchester:—

To Dr. George Webster.

Sir:—Having been, for several years, engaged in furthering the cause of medical reform, I offer no apology in forwarding to you, in your official capacity, as President of the British Medical Association, the accompanying papers relating to medical reform. Having removed from Edinburgh to Manchester in the year 1828, I soon perceived the great injustice which the Apothecaries' Act of 1815 entailed upon Irish and Scotch practitioners, and I was accordingly stimulated by that and other evils that existed in the medical profession, to memorialise the Royal College of Surgeons of Edinburgh, to endeavour to procure a reformation of the laws which pressed unequally on the members of the profession; and, as the result of our entreaties, the Apothecaries' Act Amendment Bill of 1833 was introduced by Lord Jeffrey. The subsequent steps of that Bill, and its ultimate withdrawal, owing to the obstinacy of the Edinburgh College of Surgeons, in not conceding that the man with an Edinburgh surgical diploma should, in order to be recognised as a legally-qualified general practitioner in England, have, in *addition* to that diploma, either the licence of the Apothecaries' Society, or a diploma as M.D. from a British University, are so well known that it is quite unnecessary for me to enter into any detail of the progress of the measure. The subsequent appointment of the Committee of the House of Commons on Medical Education, in 1834, and the result of their labours, are so well known to yourself, and the members of the Association over which you so ably preside, as to require no notice on my part, farther than to express my regret that no practical measure has resulted from the labours of that Committee.

Despairing of the Government or of Mr. Warburton bringing in any Bill to amend the laws relating to the medical profession, the Bill now forwarded to you was drawn up at the end of last year, and it was intended to solicit Lord Brougham to introduce that, or some similar measure, in the session of 1838. In a letter, however, which I received from Sir George Ballingall, President of the Royal College of Surgeons of Edinburgh, in January, 1838, I was informed that Sir John Campbell was expected to bring in a Bill on the subject; and as I was aware that the Edinburgh College intended to restrict *their* reform to the *recognition of the Edinburgh surgical diploma* as qualifying for a general practitioner in England, I sent a copy of our Bill to Sir John Campbell, in order that he might see the nature of other reforms the profession in England wanted, and about which the Edinburgh College of Surgeons was quite indifferent. About a month after I sent the Bill to Sir John Campbell I received a note informing me that he took a lively interest in the cause of medical reform; but as this interest was very long in being manifested, I requested my friend, Mr. Brotherton, in April, to put the question publicly to Sir John, in the House of Commons, if he intended to bring in any Bill relating to the profession in that session. The answer, you are aware, was, that he had left the matter in the hands of Mr. Warburton, but that if no Member brought in a Bill in that, he would consider it his duty to introduce a measure in the next session of Parliament. This, in my humble opinion, is mere *verbiage*, as I think Sir John cares nothing about the subject.

Sir George Sinclair, who was a member of the Medical Committee (and who has also studied the medical profession), having been in this neighbourhood in June, was waited upon, and solicited to introduce our Bill. Sir George explained the delicacy which he, as a member of the Medical Committee, was bound to show to the Chairman, and he accordingly promised to see Mr. Warburton, and if he had no measure of his own to introduce, to ask him to introduce our Bill, so that it might be read a first time and printed, in order that its provisions might be considered by the profession during the prorogation. Mr. Warburton failing to do anything, Sir George promised to introduce our Bill himself; but, as he handed our document to Mr. Warburton, who promised to examine the provisions of the measure (some of which he, viz. Mr. W., pronounced to be excellent, others to require alteration, if not entire change), Sir George considered his promises fulfilled, and his undertaking at an end. From subsequent correspondence with Mr. Brotherton, I learned that Mr. Warburton intends to bring in a Bill in the next session of Parliament, and I accordingly addressed to him the long letter (the copy of which I herewith send, with a copy of our Bill), in explanation of our views, and of the grounds on which the several clauses of that Bill were framed. I have not received any communication from Mr. Warburton acknowledging my letter, and whether he will seriously prosecute the subject in the next session of Parliament, time alone can tell.

Having thus briefly narrated the manner in which I have been engaged in forwarding the cause of medical reform, I considered it my duty to acquaint the Association with the communications that have been made to, and the documents that have been placed in the hands of, Mr. Warburton; this I considered essentially necessary, as I perceive, from your proceedings, that the Association contemplates the introduction of a Bill at an early period, and as my labours (which, I may say, have been almost *single* and *unaided*, and attended, to me, with immense *labour*, *trouble*, and *expense*) will thus be superseded, I leave the matter in the hands of the Association, in the full confidence that its enlightened members, and the Committee of Management will, from their superior facilities of getting access to Members of Parliament in London, if Mr. Warburton declines, get some Member of the House of Lords or Commons to originate a Bill on Medical Reform in the next session of Parliament.

In conclusion I have only to add, that I am still of the opinion expressed in the letter to Mr. Warburton, that it will be necessary to introduce several Bills to effect those reforms that are so necessary in the profession; but if the Association should think that a large and comprehensive measure would meet with the *support* of the Legislature, *then*, of course, it would be the most prudent step to pursue such a course.

Hoping that you will excuse the liberty I have taken in forwarding to you this letter and the accompanying papers, I have the honour to be, Sir, your obedient servant,

(Signed) MARTIN SINCLAIR, M.D.,
Fellow and Licentiate of the Royal College of Surgeons of Edinburgh.

Manchester, Nov. 12, 1838.

The letter, together with the accompanying papers, were referred to the "National Faculty Sub-Committee."

Some conversation took place relative to memorialising the Chancellor of the Exchequer respecting the duties on patent quack-medicines; but, from the lateness of the hour, the meeting adjourned without coming to a final decision.

WESTMINSTER MEDICAL SOCIETY

Saturday, November 24th, 1838.

Mr. HALE THOMSON, President.

DEATH FROM ASPHYXIA.—SIGNS OF PREGNANCY.

MR. J. READ, the instrument-maker, exhibited to the Society an ingenious apparatus, constructed on the principle adopted by M. Junod and Sir J. Murray, for the purpose of taking off atmospheric pressure from any part of the body, and inflating the lungs of persons apparently drowned. The apparatus shown presented some advantages over those inventions already before the public, in being much easier and more ready of application.

THE DEATH IN ST MICHAEL'S CHURCH, CORNHILL.

Dr. GOLDING BIRD, in answer to questions put to him by a member, stated, that his evidence on the late inquest, in which death was supposed to have been produced by carbonic acid, was incorrect, as reported in the "Times." He said, with regard to the quantity of carbonic acid which would destroy life, that all physiologists had agreed that a quantity varying from eight to ten per cent. in the atmosphere, would, if continued long, lead to a fatal result. If the quantity did not exceed ten per cent., coma was first produced, the glottis shortly afterwards closed, and the person died of asphyxia. If the quantity exceeded this, then asphyxia was at once produced.

Dr. REID read a long and elaborate paper on the fallacy of the individual signs and symptoms of pregnancy: an abstract of which we shall, perhaps, find room for next week.

Dr. JOHNSON had an opportunity lately of observing the violet or blue colour of the mucous membrane of the vagina in a pregnant woman, as first noticed by Duchatelet.

Mr. STREETER exhibited a cast taken from the breast of a woman who had had six children. The cast was taken a week after delivery, the breasts exhibiting the same appearance as they did before parturition. There was not the least appearance of follicles. The follicles were not unfrequently developed in females who had not been impregnated.

Another Member spoke at some length on the subject. The areola of virgins might vary in colour from light pink to the dark hue said by some to be an unfailing sign of pregnancy. The feeling of quickening might be produced by a moveable ovarian tumour. There were, in fact, sources of error in all the signs of pregnancy, taken individually; taken collectively they would be scarcely liable to deceive.

ANALYSIS OF FLUID CONTAINING MORPHIA.

LETTER FROM MR. HEATHFIELD.

To the Editor of THE LANCET.

SIR:—Finding my name noticed in your Journal of the 10th and 24th instant, in no measured terms, with reference to the case of the late Mrs. Tweed, I request your insertion of the following statement of facts:—

On the 25th of May last Mr. Battley, accompanied by Dr. Wheelwright, placed a

phial in my hands, informing me that it was said to contain morphia, and that Dr. Wheelwright wished to have its contents examined. The phial appeared to me to be a two-ounce bottle, and was filled with a colourless fluid.

Having evaporated this fluid to dryness, in a glass capsule, the product weighed two grains. On the addition of a drop of concentrated nitric acid to a small portion of this product, the blood-red colour which morphia and its salts exhibit, when so tested, was displayed. Mr. Battley then desired that the investigation should cease, the lady having been dead three weeks. The remainder of the dried product was carefully put aside, and on the following day returned by Mr. Battley to Dr. Wheelwright, in my presence, with a request, that he would seal up and carefully preserve it; after which the gentlemen left the laboratory together, and I presume that Mr. Battley stated his reasons for not carrying the investigation further at that time. Dr. Wheelwright called on Mr. Battley, after the lapse of five months, and requested to be furnished with a certificate of the examination, which Mr. Battley declined giving, but directed me to make a copy of the notes on the subject, entered at the laboratory.

The dried substance which was delivered to Dr. Wheelwright, or some part of it, is understood to remain in that gentleman's possession, and if so, the pure morphia can, of course, be separated if required. I am, Sir, your obedient servant,

W. E. Heathfield.

Laboratory, Cripplegate,
Nov. 26, 1838.

DOSE AND PREPARATION OF MORPHIA.

To the Editor of The Lancet.

Sir:—In answer to your question about the dose and mode of administering the acetate of morphine, I beg to state to you the preparation which I have used very extensively during the last few years. It is convenient in form, and prevents the possibility of any mistake occurring in the dose. I dissolve eight grains of acetate of morphia in eight drachms of distilled water, slightly acidulated with acetic acid. To this solution I add five drops of spirits of wine, and keep the bottle carefully corked for use.

The usual dose which I direct of this preparation is twenty minims, but, frequently, if necessary, thirty, or forty, or in spasmodic diseases, frequently, much more. I never knew any ill effects produced by morphia, which I esteem one of the most valuable medicines we possess. In ordinary cases, twenty drops will produce a soothing sedative effect, subduing irritability, and causing refreshing sleep. If the patient be suffering from much pain, or have taken anodyne medicine before, thirty drops will be necessary, and it will seldom be requisite to increase this dose, which I never saw produce narcotism, or any other bad effect.

I perfectly agree with the observations which you have made upon Messrs. Battley's analysis. According to their results, and supposing half a grain to have been lost in the analysis, there must have been originally *five grains* in the draught (afterwards divided into two portions) prepared by Mr. Arden. Now the acetate of morphia is *exceedingly light;* and let any person weigh out five grains, or even four grains, and compare it with one grain. The disproportion would be so great, and the former quantity so unlike the grain of any other substance, that I believe the mistake attributed to Mr. Arden *impossible* to have occurred.

I am, Sir, your obedient servant,

C. R. Bree

Stowmarket, Nov. 29, 1838.

To the Editor of The Lancet.

Sir:—In the last Lancet, your ask a question respecting the largest dose of acet. of morphia exhibited. In compliance with the request I give, from memory, the following case, which occurred in my wife, and I can assure you is correct.

Mrs. Dickinson, aged forty-six, first complained of stiffness in the knees, and difficulty in coming down stairs. In a few weeks she could not trust herself to walk alone. At this time I took her to Sir A. Cooper. She had violent muscular contraction of both legs, if the limbs were flexed, but if unbent the spasm extended the limbs, the great toe being brought nearly into a straight line with the patella. These spasms were of the most painful kind, to subdue which I first gave the acetate of morphia, in quarter-grain doses, but latterly the muriate. She was also, after a time, seen by Mr. Lawrence. Every plan suggested was carefully tried, but the case eventually terminated fatally, after nearly two years' suffering. During the latter months the afflicted patient took from twelve to fourteen grains of the muriate of morphia, in divided doses of two or three grains. It was given *ad libitum*, so as to subdue the spasm. This quantity was given every twenty-four hours. I may observe, in conclusion, that paralysis extended from the first dorsal vertebra to the back. The moxa, large issues, &c., were repeatedly applied. No smaller dose of the muriate of morphia would have kept her in anything like comfort or free from pain. I remain, Sir, your obedient servant,

Geo. Dickinson.

Ealing, Nov. 27th, 1838.

MR. SIMPSON AND DR. GREGORY.

To the Editor of THE LANCET.

SIR:—By the insertion of my letter on the subject of "the officials at the Small-Pox Hospital," I have every reason to believe that you have done good. To every candid and unbiassed reader, Dr. Gregory's attempt at reply must be only "amusing," as he does not grapple with a single charge which I brought forward. I repeat, that I can *prove* my assertions by the testimony of other medical men, who reside nearer to "St. Paul's" than either "Mile-end" or "Hyde-park-corner;" and should it be necessary to mention their names they have volunteered to give them. I am, Sir, your most obedient servant,

W. SIMPSON.

111, Guildford-street,
Nov. 20, 1838.

*** We omit from Mr. Simpson's letter some remarks at the close, which were probably made in compliance with the challenge of Dr. Gregory, to have "another bout." We can see, from the whole correspondence, that the controversy might very soon become altogether personal, and believe that Mr. Simpson will willingly allow us to confine his observations to the question immediately at issue.—ED. L.

POST-MORTEM APPEARANCES OF JAMES TRICKEY.

To the Editor of THE LANCET.

SIR:—Some differences of opinion having been expressed by the medical witnesses, at the inquest held upon the body of James Trickey (who was found dead in St. Michael's Church, Cornhill), as to the cause of death, I beg to forward you a plain, unvarnished statement of the appearances presented at the post-mortem examination, which I trust you will insert in Saturday's LANCET, for the consideration of the profession and the public at large, that they may, one and all, be impressed with the urgent necessity of medical coroners. I am, Sir, your obedient servant,

JAMES JOHN GREER, Surgeon,
8, St. Thomas's-place, Hackney.

Sunday, Nov. 18, 1838, half-past 2, P.M.—Attended, in company with Mr. South, Assistant-Surgeon of St. Thomas's, Dr. T. Bull, Mr. Pugh, Mr. Smith, Mr. Blenkarne, and others, to examine the body of a man in St. Michael's Church, who had been found dead at a quarter past seven that morning. The body was removed from the shell and placed upon forms raised in the cloister. On examining the body the face was a little flushed; the eye natural, pupil somewhat dilated; the jaws were about half an inch apart; the tongue lay straight, and appeared natural, it did *not* protrude beyond the teeth, nor was it at all swollen; the lips appeared natural, not at all blue; no spots or marks appeared about the face or chest; there was a slight bruise over the right temple; face full; very short neck; the abdomen not at all distended; he appeared to have suffered from umbilical or omental hernia. The head was first examined; on sawing through the temporal bone the saw entered the substance of the brain, when there was an escape of three or four ounces of serum, mixed with blood, which had escaped from the vessels lacerated by the saw; the vessels of the brain were congested; great effusion between the arachnoid and pia mater. In cutting into the substance of the brain the vessels were very turgid, and full of blood; *florid-red blood* followed every incision, and that in great quantities; the lateral ventricles were nearly half full, and bore the appearance of having been distended with serum (I would here observe, on attempting to draw back the cap, the dura mater was so closely adherent that the brain was raised at its base, whereby a large escape of serum took place); in the lateral ventricles were observed four or five small coagula; the cerebellum bore the same appearance on being cut as had been observed in the cerebrum; the tunica arachnoidea appeared thickened; no cyst was discovered in the substance of the brain; there was great effusion of serum throughout the convolutions. Mr. South inquired if any gentleman present wished to pursue the inquiry further in examination of the brain; all appeared to me to be of one opinion, that the deceased died of serous apoplexy. We then proceeded to examine the chest; on cutting along the median line the pectoral muscles appeared of a very florid colour, and firm. An attempt was made by Mr. South (at the suggestion of Mr. Smith) to tear them without effect. On removing the sternum the lungs appeared very dark and turgid. To examine them the better, it was proposed to tie the vessels and œsophagus, and remove them, with the heart, into the light, that they might be more accurately examined; having done so, the lungs appeared very dark, having a greenish hue, firm, and turgid; on tearing them there was an abundance of mucus. We then returned to the cloister to examine the heart, &c. The pericardium was imbedded in fat; the right ventricle was first cut into, and appeared natural, being about one-third full, and having one or two small coagula; on cutting into the left ventricle, its walls, or muscular structure, were very thick; there was some blood in the left ventricle, and a large coagulum; the mitral valve was ossified in part; the root of the aorta appeared to *me* somewhat enlarged, or larger than usual;

the trachea was opened its full length; it was lined with mucus, quite white, and free from admixture with blood; the posterior part of the lining membrane appeared rather turgid in places; the stomach was large, and nearly three parts full; it had the usual gastric smell, nothing unusual or particular; it contained several pieces of meat, undigested, which appeared like cold boiled mutton; there were a few streaks of redness about the stomach, and it appeared somewhat irregular on its surface; the diaphragm was coated with fat. I never remember to have witnessed so great a deposition of fat in any subject before. On dividing the integuments of the abdomen it appeared the deceased had suffered from omental hernia; the deceased had voided his urine and fæces; the blood on the boards on which the deceased was laid *coagulated* by exposure to the air.

DEFENCE BY DR. SHEARMAN OF THE AWARD OF THE LAST FOTHERGILLIAN MEDAL.

To the Editor of THE LANCET.

SIR:—I happened to see in your Number of the 10th instant, a statement given on the authority of "A. Z.," the apparent intention of which is, to cast obloquy on the Medical Society of London, respecting its proceedings in bestowing the Fothergillian medal in March last.

It is very convenient for any one bringing forward false charges and unfounded accusations to skulk behind an anonymous signature, by stating a part only of the truth, and suppressing so much of it as would elucidate the whole transaction, to raise a a temporary suspicion of unfairness of conduct.

It is not to be expected that a respectable body, like the Medical Society, would enter the arena to defend itself from unfounded charges brought against it by every anonymous scribbler. But as an individual member of that Society, intimately acquainted with all its concerns and all its proceedings, I have no hesitation in stepping forward to answer whatever charges your correspondent may choose to allege against it. If he will boldly avow himself, and thereby place us upon an equal footing, I have little doubt of being able to prove how utterly unfounded is the imputation attempted to be conveyed by his artful insinuation.

In the mean time, I would recommend "A. Z." to read the successful paper, and determine for himself whether its intrinsic merits do not entitle the author to the modicum of reward bestowed upon it. Of the comparative merits "A. Z.," of course, cannot know anything, unless he was an unsuccessful candidate, in which case his opinion might not, perhaps, be an impartial one. If he was an unsuccessful candidate, let him publish his paper, and the medical public will have an opportunity of fairly judging between the two.

I do not understand what "A. Z." means when he says, "if the present rule is to be taken for an example." I know of no present rule different from the rule invariably acted upon from the very foundation of the prize. The adjudication is entrusted to six or seven gentlemen of unimpeachable integrity, who are appointed immediately after the anniversary, many months before a single paper on the prize question is ever sent in. The medal is awarded to the paper (author unknown) which appears to be the best in the judgment of the majority of those of the six or seven who are *present* at the adjudication. If it is any satisfaction to your correspondent, I can inform him that the author of the successful paper was not present at the adjudication, and, consequently, had no more a voice in the matter than your correspondent himself. That the very best paper among those offered for adjudication should afterwards prove to have been written by one of its own members, can never, I think, be considered as a disgrace to the Medical Society of London. I am, Sir, &c.

WILLIAM SHEARMAN.

Northampton-square, Nov. 17, 1838.

THE TERM "PRACTICAL CHEMISTRY" AT APOTHECARIES' HALL.

To the Editor of THE LANCET.

SIR:—In the last curriculum published by the Society of Apothecaries it is required that every student shall attend a course of practical chemistry, which command has been understood by lecturers in a manner that might have been anticipated, and courses of practical chemistry, distinct from the general chemical lectures, have been instituted at most of the London schools.

It occurred to me as being an anomaly in the regulations of any examining body, that a term so loose as "practical chemistry" should be used, without stating the extent of its purposed application; and as I felt a little interested in the matter, I applied to the authorities at the Hall for information, when, much to my surprise, I was informed that the Hall never contemplated enforcing the study of *practical*, as a subject distinct from *general*, chemistry, but it was merely intended that every lecturer should make pupils acquainted with the method of conducting chemical manipulations, during the progress of the usual chemical lectures.

This communication certainly surprised me not a little; and I asked if a similar misunderstanding of the curriculum had not occurred to other people besides myself? I

was answered in the affirmative, and that, in consequence of a disturbance in one of the Borough schools, arising from an unwillingness on the part of the students to pay the disputed fee, the lecturer at that school proceeded to the Hall for instruction, and eventually compromised the difficulty by altering the diction of his prospectus.

Now, if pupils be burdened with the payment of a fee of three or four guineas *by mistake*, surely such a mistake should be corrected. Having received a verbal assurance from the Hall authorities to the effect that pupils on entering to their chemical lectures were entitled to instruction in practical chemistry, without the payment of any additional fee, I wished to have the same assurance *in writing*, that I might publish it for the benefit of parties concerned. Under this impression, and believing the course which I was pursuing to be such as would be acquiesced in by any party anxious to correct an error, I addressed a letter (dated Oct. 12) to the solicitor of the Worshipful Society of Apothecaries. But I have received no answer, and such being the case, it only remains for me to afford that information on my own credit, which I hoped to have been able to transmit in the form of a letter written at the source of error. I am, Sir, your obedient servant,

JOHN SCOFFERN.

10, President-street, West,
Nov. 17, 1838.

FURTHER TESTIMONY OF THE REMEDIAL VALUE OF THE

CARBONATE OF SODA.

To the Editor of THE LANCET.

SIR:—In common with Dr. C. Selwyn and Mr. Carter, I hope you will allow me, in the next number of THE LANCET, to enter my caveat against the reception of M. Magendie's opinions with respect to the action of the carbonate of soda on the blood.

I have long prescribed it,—frequently in large doses; and without enumerating its very beneficial action in typhus fever, in dyspepsia, certain states of anæmia, &c., I will content myself with pointing out its great value in the treatment of phthisis. Some of the numerous cases in which I have prescribed it are on record in my recently published work on consumption and scrofula; and, so far from its having caused liquefaction of the blood, infiltration of the lungs, or pneumonia, I think I have great reason to laud its tonic effects in removing crude tubercles, as well as in obviating that state of superabundant carbon in the blood, and that sluggish condition of it, which I believe to obtain in phthisis, in some of its stages, and the existence of which I have, in that work, endeavoured to show. It has been especially useful in threatened cases of tubercular infiltration. It has not even appeared to increase the frequency of that intercurrent pneumonia which is so often observed to occur in phthisis still less to give rise to it; indeed, my experience would incline me to support the very reverse propositions.

The direct injection of the carbonate of soda into the blood is one thing; but the introducing it into the stomach is another; and whether or not it there meets with free hydrochloric acid, or is otherwise changed, the ultimate effect, as regards the blood, must be very different from that of direct injection; at all events, similarity or identity of effect must not be hastily assumed.

The name of M. Magendie, as a physiologist, is so high, and some of his experiments are so brilliant, that every one who has experience ought the more readily to come forward to show any falsity in his facts or error in his theories, that may bear upon practice; and as I highly applaud his distinguished experimental efforts, this motive forms the best apology for this intrusion on your pages from, Sir, yours most obediently,

J. J. FURNIVALL, M.D.

Hertford, Nov. 18, 1838.

ON THE

DIVISION OF THE PROSTATE IN LITHOTOMY.

By H. M. PHILLIPPS, *Esq., Assistant-Surgeon to the Royal Cornwall Infirmary.*

A SENSE of duty induces me to communicate to the profession, through THE LANCET, a fact of some practical importance, which I hope will tend to diminish the risk of life usually attending the lateral operation of lithotomy, the dangers to be apprehended from which are, hæmorrhage, puncture of the rectum, peritoneal inflammation, with purulent deposition about the neck of the bladder, and infiltration of urine with its consequences. All these may be avoided, I consider, by adopting the following *modification* of the operation, which I strenuously recommend to all operating surgeons:—

Having introduced a straight-grooved staff into the bladder, and having reached the membranous portion of the urethra, by the usual incisions on the left side of the perineum, I cut into the groove of the staff. The staff being still firmly held by an assistant, I introduce the nail of the forefinger of the left hand into the groove, then insert the point of the knife also into the groove, in advance of the finger, its flat surface resting on, and being parallel to, the plane of the nail. Both are then carried steadily onward, until the knife enters the

bladder, indicated by the gush of water. It is then withdrawn, and *the finger* alone is pushed firmly and fairly into the bladder. The forceps is then introduced, upon the finger (the best director in all operations), and the stone is embraced.

It will be seen that the principle acted upon here, is the same as that which proved so successful in the hands of Cheselden, and was so warmly commended by Sir Astley Cooper, namely, the partial separation of the upper from the lower portion of the prostate gland with the knife, completing the separation to the necessary extent without a cutting instrument. Cheselden used for this purpose the blunt curved gorget. I use *the finger*, and having tried this method on the adult, I have found no difficulty whatever in enlarging the opening sufficiently by simply protruding the finger into the bladder, which is accompanied with the sensation of a slight tearing.

The advantages of this mode of operating are, the certainty of avoiding hæmorrhage, or of puncturing the rectum, and the equal certainty of being able to make the opening into the bladder large enough to extract the stone, and no larger. I may add, that I never yet found any perineum too deep to prevent my enlarging the section of the prostate with the finger, and I am quite satisfied that any lithotomist who may adopt this method will not readily abandon it.

Truro, Nov. 22, 1838.

CHEMICAL EXAMINATION OF THE LIQUOR AMNII.

BY G. O. REES, M.D., F.G.S, &c.

This secretion has been examined by several continental chemists, with a great diversity of result, not only as to the quantities, but likewise the quality of its constituents. Buniva and Vauquelin make its specific gravity 1004; and its solid contents, 11 to 12 parts in 1000. Frommhertz's experiments differ from the late observations of Dr. Vogt, of Bern; who attempts to explain the discrepancy, by supposing that the fluid, as examined by Frommhertz, was not obtained pure.

In a Paper on Albuminous Fluids, by Dr. Bostock, published in the fourth volume of the "Medico-Chirurgical Transactions," the solid content of a specimen of liquor amnii is given as 16.6 in 1000 parts; which would make the specific gravity of the fluid to be about 1008.6. This observation is in close accordance with that of Dr. Rees; the four specimens which he examined varying in specific gravity between 1007 and 1008.6. The analyses were performed on specimens of liquor amnii with which no extraneous matter was admixed, the liquor being drawn off either by a canula or a female catheter.

C. W. has been delivered of three children. In the two first labours, the perforator was employed to effect delivery. The third labour was brought on at 7½ months; the child born dead. On examination, the brim and outlet of the pelvis were found to be contracted; labour was, therefore, induced at 7½ months, by passing a female catheter into the os uteri, and drawing off the liquor amnii. Labour pains came on twenty-four hours after the operation, and in forty-one hours a female child was born alive; it lived only half an hour.

EXAMINATION OF LIQUOR AMNII.

Strongly alkaline.—Sp. grav. 1008.6.

Contained in 1000 parts:

Water	983.4
Albumen (traces of fatty matter)	5.9
Albuminate of soda } Chloride of sodium }	6.1
Animal extractive soluble in water and alcohol, urea, chloride of sodium	4.6
Traces of alkaline sulphate.	

The examination of a second specimen gave in 1000 parts:

Water	984.98
Albumen (traces of fatty matter)	1.80
Extract soluble in water—Salts, 2.80; Organic matter, principally albumen, from albuminate of soda, 3.22,	6.02
Extract soluble in water and alcohol—Salts, 2.80; Organic matter, principally lactic acid and urea, 4.4	7.20

The third specimen examined was taken from the same patient as specimen 1. The liquor was abstracted in the same manner, and at the same period of utero-gestation (7½ months). The fœtus was a female, and still-born. Contained in 1000 parts:

Water	986.8
Albumen (traces of fatty matter)	3.2
Aqueous extract—Salts, 3.2; Organic matter, viz., albumen, from albuminate of soda, 1.2	4.4
Alcoholic extract—Salts, 3.8; Organic matter, viz., lactic acid and urea, 1.8	5.6

In the fourth specimen were:

Water	986.8
Albumen (traces of fatty matter)	2.4
Aqueous extract—Salts, 4.2; Organic matter, viz., albumen, from albuminate of soda, 2.0	6.2
Alcoholic extract—Salts, 3.0; Organic matter, viz., lactic acid, and urea, 1.6	4.6

The fatty matter procured from the liquor amnii differs from that of the blood, in not assuming a deep purple tint when digested in strong sulphuric acid.

The salts, both of the aqueous and alcoholic extracts, consisted of chloride of sodium and carbonate of soda, with minute traces of an alkaline sulphate and phosphate. In the salts of the aqueous extractive, the carbonate resulted from the incineration of an albuminate; and in the alcoholic extractive, from the decomposition of a lactate, by the same operation.

The salts obtained from the aqueous extractive in two analyses were not entirely soluble in water. The insoluble matter, on examination, proved to be phosphate of lime, which must either have been held in solution with the albuminate of soda, or have resulted from the decomposition of an alkaline phosphate at a red heat; some soluble earthy salt being present to effect such decomposition.

Urea was proved to exist in the liquor amnii, by its characteristic crystallisation with nitric acid, and the re-actions of its nitrate. A further evidence was obtained in the analysis of specimen 3 by the crystallisation of chloride of sodium in octohedra, from the alcoholic extract, when allowed to evaporate spontaneously. Specimen 3 contained urea in greater proportion than the other specimens.

The albumen procured from the liquor amnii yields a trace of oxide of iron and earthy phosphate, on being incinerated.

The flocculi which are observed floating in the liquors are composed of caseous matter, containing cholesterine.

On examining the analyses, it will be observed that the liquor amnii varies greatly in proportional constitution in different individuals, at the same period of utero-gestation, which shows that, like, perhaps, all the secretions of the body, it is affected by the temperament and diathesis of the mother. The specific gravity of the secretion, however, varies but little in the four specimens, which is possibly a precaution on the part of nature to preserve a medium of fixed power, to oppose the motions of the fœtus in utero.

Whether or not the liquor amnii varies in concentration at different periods of utero-gestation, remains to be shown: it is certainly pretty constant at 7½ months.

The experiments of Dr. Vogt, of Bern, would lead us to suppose that there is great variation in the density of the fluid at different periods. At six months, that chemist asserts that he has found 9.71 parts of solid matter in 1000 of the liquor; and at 3½ months, 20.55 parts in 1000. There is, however, a want of proper relation between the solid content and specific gravity of the fluids, as given by the doctor, that prevents Dr. Rees from regarding his experiments as conclusive.—*Abridged from Guy's Hospital Reports.*

SYMPTOMS OF POISONING FROM AN ORDINARY DOSE OF LAUDANUM.

In a late Number we gave the history of a case in which the administration of a moderate dose of opium was supposed to have produced a fatal effect. The following case shows that even a still smaller dose will sometimes give rise to symptoms of poisoning:—

Madame G., 29 years of age, had been in the habit of administering to herself laudanum in lavements, during several months, for the purpose of assuaging uterine pain. For the last six days she had taken twenty drops of laudanum every evening, and had slept quietly. On the 25th of February Madame G. took her usual lavement, and in five minutes was seized with vertigo, numbness in the limbs, and difficulty of speaking. After the lapse of ten minutes more, the face became pale, the extremities cold, the pupils dilated, she felt a sensation of choking, the pulsations of the heart became extremely accelerated, and the patient lay under the apprehension of imminent death. Purgative clysters; sinapisms to the hands and feet; draught with ether.

After an hour the palpitations had somewhat diminished, and the extremities became less cold. The patient showed a strong tendency to sleep, but on dozing she started up and evinced great incoherency, both in language and actions.

On the following morning, at nine o'clock, the pupils were still extremely dilated, and the frequence with irregularity of the pulse still continued. The patient was now bled at the arm, but without any relief. The delirium continued for twenty hours; and the sensation of numbness in the limbs did not disappear before the lapse of forty-eight hours. The dilatation of the pupils was the symptom which continued the longest.—*French Lancet*, Nov. 6, 1838.

FRENCH EXAMINATIONS.

The proportion of students rejected to those examined by the Apothecaries' Hall and College of Surgeons, has been established by returns ordered by the House of Commons. The number of rejections amongst a given number of students cannot be regarded as a standard of professional attainment; nevertheless, the following statistic may be interesting as a point of comparison. During the year 1837-8, 2394

French pupils were examined at the different faculties, and of these 353 were rejected, being a proportion of one in 6.52. The French students undergo five distinct examinations, and the proportions of rejected at each examination were as follows:

First Examination.

Natural philosophy, natural history, and chemistry:—Examined, 193; refused, 39.

Second Examination.

Anatomy and physiology:—Examined, 640; rejected, 174.

Third Examination.

Medicine and surgery:—Examined, 567; rejected, 114.

Fourth Examination.

Medical jurisprudence, materia medica and therapeutics, hygiene:—Examined, 474; rejected, 67.

Fifth Examination.

Medical and surgical clinique, and midwifery:—Examined, 423; rejected, 29.

French Gazette, Oct. 6, 1838.

CHEMICAL NATURE OF THE SECRETIONS AND DISTRIBUTION OF NERVES TO SECRETORY ORGANS.

RECENT researches of M. Douné have led him to conclude that the secretion of all organs, which receive their nerves from the cerebro-spinal system, is alkaline, while all organs which receive their nerves from the ganglionic system produce an acid secretion. The following table of the secretory organs, their nerves, and the chemical properties of their secretions, gives a summary view of this doctrine:—

Secretion.	Property.	Nerves.
Saliva	Alkaline	5th (R. 111) and 7th nerves.
Tears	Alkaline	5th (R. 1.)
Mucus of Nose	Alkaline	Olfactory; non-ganglionic filaments of fifth.
——— Pharynx	Alkaline	Vagus; glosso-pharyngeus; fifth.
——— Stomach	Acid	Cæliac plexus.
——— Intestines	Acid	Sympathetic.
——— Uterus	Acid	Sympathetic.
——— Vagina	Acid	Ditto.
——— Bladder	Alkaline	Sciatic plexus and some filaments of the sympathetic.
Urine	Acid	Renal plexus.
Pancreas	Acid	Sympathetic.
Milk	Alkaline	Intercostal nerves.
Liver	Acid and Alkaline	Hepatic plexus, and eighth pair.

Retzius, and, since him, Müller and Remak, have demonstrated the presence of some grey fibres of the ganglionic system, in the midst of the cerebro-spinal nerves. The presence of these filaments explains the small quantity of acids found in the alkaline secretions, and which contributes to the formation of their salts. On the other hand, the existence of some filaments of the cerebro-spinal system in the ganglionic, explains the presence of an alkali, as soda, &c., in acid secretions. The only exception to the above rule is the perspiration, which is acid. —*French Lancet*, Nov. 10, 1838.

UNQUALIFIED PRACTITIONERS IN MEDICINE.

ALLEGED NEGLECT OF THE SOCIETY OF APOTHECARIES.

To the Editor of THE LANCET.

SIR:—Can you give us any information respecting any Act of Parliament which may have been passed since that of the 55th George III., for "the better regulation of apothecaries," &c.? It is respectably reported here, that individuals who have attended lectures at any public dispensary have the approval of the Worshipful the Society at Blackfriars to practise,* and that a case has been sent up, and an answer returned, that the Society will not prosecute any one so acting. Pray, Mr. Editor, do throw some light on this subject. Until we see the fact confirmed in one of your forthcoming Numbers, the statement must be doubted.

A young man, in business here as a chemist and druggist, and who served his time

* Do our correspondents mean to state that the Company of Apothecaries have been in the habit of examining persons who have not been qualified according to law to be admitted as candidates for the licence? We should like to have a distinct answer to the question, and if it be in the affirmative, we also request to be furnished with a statement of the facts which can be adduced to sustain the allegation.—ED. L.

to the said business, is about commencing practice under the authority and sanction of the medical man who attends the sick-poor at the dispensary, where the former either attended the practice, or the lectures, for a limited period. Who would incur the expence of medical certificates, and pay the fees at Apothecaries' Hall, when a few weeks, and as many lines, only, are wanted to qualify an individual to treat disease in all its varied forms? I am a Member of the College, and a Licentiate of the Hall, and am, with many others, disgusted at the state of things in the profession. I am, Sir, with feelings of admiration for what you have already done to uphold the respectability of the profession, your most obedient servant,

A Practising Surgeon.

Bristol, Nov. 26, 1838.

, The foregoing letter having been authenticated by the signature of two respectable surgeons, we think it to be our duty to inform those gentlemen that, in our opinion, the report which they have heard, respecting the conduct of the Society of Apothecaries, is a calumny on that body. Such practitioners as those who are described by our correspondents are, to all intents and purposes, unqualified persons, and might, without doubt, be prosecuted to a conviction, under the 55th Geo. III. There exists no Act of Parliament to relieve such impostors from the penalties which are imposed by that statute.

If our correspondent should obtain any more information on the subject of his communication, we shall be glad to hear from him again, and will take care that the Society of Apothecaries is also placed in possession of it. The profession shall then learn whether that body be really disposed, or not, to check the career of unqualified pretenders, and thereby protect the interests of regularly licensed practitioners.

DR. A. T. THOMSON'S LECTURE ON HYPERTROPHY OF THE HEART.

To the Editor of The Lancet.

Sir:—It occurred to me, on reading the clinical lecture of Dr. A. T. Thomson, published in The Lancet of the 24th inst., that the manner in which it was expressed would tend to mislead your readers as to the Doctor's views. Dr. Thomson did not *mean* that Gardner was discharged "apparently well" of his hypertrophy, but simply of his dropsical symptoms. The hypertrophy of his heart, with its abnormal sounds, so *accurately described* by Mr. Taylor, the apothecary, of course remain, and the *dilatation* is also undiminished. It must, therefore, be obvious that in proposing this new plan of treating hypertrophy of the heart, Dr. T. did not mean to imply that it was shown to have any influence in *curing*. You will, of course, give this note immediate insertion, as the lecture forms a matter of great conversation. I am, Sir, yours, &c.

A Pupil.

University College Hospital,
Nov. 27, 1838.

MERCURY WITH CHLORIDE OF SODA.

To the Editor of The Lancet.

Sir:—In reply to the question of your correspondent, Mr. Samuel (Lancet, No. 3, present Vol.), whether the severe effects of the mercury in his cases are to be solely attributed to a peculiar idiosyncracy, or to the simultaneous exhibition of the chloride of soda, I beg to state my opinion, that it is to be attributed to the latter only, as I have had occasion frequently to observe the increased effects of small doses of mercury, when administered at the same time as the chloride. I am, Sir, your obedient servant.

W. B. Dickinson, M.D.

Marazion, Cornwall, Nov. 5, 1838.

INTELLECT AFTER LOSS OF MUCH BRAIN.

To the Editor of The Lancet.

Sir:—Your valuable Journal of Nov. 3, contains a communication from Mr. Harding Drawbridge, according to which a girl had continued to enjoy the *entire* use of her intellectual faculties for four days after the loss of a considerable portion of the cerebral mass. You are, of course, aware that such a phenomenon *is* reconcileable with the doctrines of phrenology, *provided the injury be confined to only one of the hemispheres.* Your correspondent would, therefore, confer an obligation on your numerous readers by informing them *whether*, and to what extent, *both* the hemispheres were affected. Could Mr. Drawbridge, at the same time, offer some observations respecting the amount of intelligence exhibited, and also what particular phrenological organs were injured, his information would acquire additional value. Yours respectfully.

A Lecturer.

Newington, Nov. 6, 1838.

EIGHT POUNDS A YEAR!

To the Editor of THE LANCET.

SIR:—In the "Times" newspaper of the 2nd Nov. appeared the following advertisement:—

"To MEDICAL ASSISTANTS. — Wanted a young man who *thoroughly understands dispensing medicine*, and will not object to carry it out. Salary, £8 a year, with board and lodging. Apply, any morning before 10 o'clock, at 38, Berners'-street, Oxford-street."

Can any member of the medical profession have issued this announcement? Blush, reader, to learn that the question must be answered, *yes*. The deepest poverty can alone induce a medical assistant to accept such an offer, and such, I trust, is the feeling of honour for themselves, amongst the present race of students, that not one will be found who will not rather be contented with an humble pittance from any honest daily labour, than become the servile menial of a person who could offer him this alternative. I refrain from exposing the name of the advertiser; his conduct merits only silent contempt; but sincerely do I hope that this notice of his conduct will rouse him to a sense of propriety, and induce him to stop the appearance of advertisements (very frequent of late) which are so derogatory to the respectability and the best interests of the profession. I am, Sir, your obedient servant,

MEDICUS.

Peckham, Nov. 13, 1838.

The person from whom the liberal announcement emanated is named Tucker.—ED. L.

BOOKS RECEIVED.

A General Outline of the Animal Kingdom. By Thomas Rymer Jones, F.Z.S., &c. Van Voorst, London.—[This work promises to furnish a useful view of the animal creation to the general reader.]

The Objects and Mutual Relations of the Medical Sciences; an Introductory Address, &c. By F. S. Leighton, M.D., &c. Renshaw, London.

A Treatise on the Venereal Disease and its Varieties. By Wm. Wallace, M.R.I.A. New edition, with plates. Renshaw, London, 1838.—[The value of this edition is much enhanced by the addition of some curious plates, which illustrate the primary forms of syphilitic sores.]

The Chirurgico-Comico Alphabet. By Pill-box. Renshaw, London, 1838.

Practical Observations on the Causes and Treatment of Spinal Diseases. By Samuel Hare, Surgeon. Simpkin and Co., London, 1838.

The Elements of Materia Medica. Part I. By Jonathan Pereira, F.R.S. &c. Longman and Co., London, 1839.—[We defer noticing this valuable work until the second part shall have appeared.]

Treatises on Physiology and Phrenology, from the Encyclopædia Britannica. By P. M. Roget, M.D. 2 vols. Black, Edinburgh, 1838.

An Epitome of Chemistry, &c. By J. Barker, Practical Chemist. Portsmouth, 1838.

A Text-Book of Human Anatomy. By Robert Hunter, M.D. Second Edition. Robertson, Glasgow, 1838.

An Essay on the Comparative Effects of Human Respiration, and of Combustion of Charcoal, upon the Atmosphere. By C. T. Coathupe. Philip and Evans, Bristol, 1838.

An Historical View of Vaccination. By Vigorniensis. Rivington, London, 1838.

A Letter on the Present State of the Law of Lunacy, &c. By a Barrister of the Inner Temple. Crofts, London, 1838.

TO CORRESPONDENTS.

The letter of *Candidus* is expressive, throughout, excepting in one or two lines, of individual opinions. He says, "*I* reprehend," "*I* disclaim," "*I* have always been of opinion," "*I* think," "*I* consider," "*I* am unable," "*I* need not remind," "believing, therefore," "*I* do not advance," "*I* cannot decide," "*I* have read," "*I* have much regard," "*I* am compelled to denounce," "*I* should feel both shame and fear." *Candidus* expressly claims the insertion of his letter. What force can anonymous *opinions* have in the matter? Arguments may be weighty from any quarter. Mere denouncements need the authority of a name.

Dr. Wilks's letter next week.

When our friend, *A Philosopher*, recovers his lost senses, we shall be happy to hear from him again.

We feel obliged for the observations forwarded on Dr. Burne's paper.

If *Tyro* will employ the remedy himself, we shall be happy to record the results of his experience.

Mr. Andrews will find his communication in the last number of THE LANCET.

Communications have been received from *Messrs. Lowther, A. Walker, Bodington*, and *G. Dillon*.